The Journal of the
American Medical Association

VOL. XVIII. CHICAGO, MAY 7, 1892. No. 19.

ORIGINAL ARTICLES.

THE ESOTERIC BEAUTY AND UTILITY OF THE MICROSCOPE.

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From the Microscope, January, February and March, 1892.
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Three things prompt the writing of this paper:

1. It is the tri-centennial of the microscope.

2. It has been stated that not much has been done with the microscope in America (1891).

3. At a meeting of the Presbyterian Club of New York, a few years ago, a divine repeatedly alluded to the "accursed microscope." This man represented himself. It is a libel on his church to think such a thing. It is a patron of science, microscopy included.

Now the writer feels that he owes a debt of gratitude to the Dutch for inventing the microscope—that America has done and is doing something with the microscope of great practical value, and that things of beauty and utility should not be called *accursed*. Hence he begs to testify what he knows in his own practical, every-day experience. While giving due credit to others, he holds himself only responsible for what is here asserted, like a witness in court. The term esoteric is used to express the inwardness of this beauty and utility as a matter of fact, in the hope that they may become exoteric, and benefit others as much as they have the writer. The beauty is that grace and feature of excellence of the microscope that pleases the mind as a *means to an end*. The microscope is not an end. It is a great mistake to think it is an end.

The Microscope in Disease.—I use it to examine the blood, sputum, fæces, urine, skin, secretions of eyes, ears, nose and glands, and vomitus.

This morphological examination does not exclude other physical exploration or chemical examination. It supplements them, and with them forms a most valuable basis for the detection and treatment of pathological conditions.

For the sake of brevity, please let *Morph.* A = The morphology of the blood.[1] *Morph.* B = The morphology of the sputum, which may include all discharges from the air passages, which are expectorated through the mouth. *Morph.* C = The morphology of the secretions and other objects found with the use of the microscope on the skin, specially in the sweat and dirt. *Morph.* D = The morphology of the fæces. *Morph.* E = The morphology of the urine. *Morph.* F = The morphology of the milk. *Morph.* G = The morphology of the secretion of the eyes. *Morph.* H = Morphology of the secretion of the ears. *Morph.* I = Morphology of the secretion of the nose.

Consumption—Phthisis Pulmonalis. Morphs. A, B, D, E.—The microscope is a means to the following ends in the diagnosis of: 1. The pre-tubercular state any time within a year before the lungs break down, which forms tubercular. 2. The tubercular state. 3. To distinguish between consumption of the lungs and bowels. 4. Between consumption and bronchitis and asthma. 5. Fibrous consumption. 6. Consumption of the blood. 7. Between consumption and uterine disease. 8. Bright's disease of the lungs. 9. Consumption in cases of life insurance. 10. To know how the patient gets on under treatment. 11. To know when to add to or restrict diet. 12. To know when the patient is cured.

Remarks.—Sometimes, to distinguish elastic or inelastic lung fibres in the sputum, the polariscope must be used, as cotton, linen or woolly fibre may be changed by the action of the sputum so as to closely resemble the lung fibres, which do not polarize light. No real progress to health is made in any case before the normal morphologies take the place of *Morph. A, B, D, E.* The microscope alone reveals these, hence what a useful and beautiful thing it is to settle so many points that are a great trouble to physicians. To give an idea here of these abnormal morphologies would take too much time and space. Indeed, they are best taught by practical lessons from some one who understands them. These ideas are not wholly original with the writer. They are American, and have been in use for many years. The writer reported at the Tenth International Medical Congress, Berlin, one hundred cases of consumption; forty of them were cures, mostly of ten years' standing; some of twenty-five years. The treatment has been published in the Trans. Am. Med. Association, 1880. From these we infer that the Netherlands are to be much honored for the discovery of the microscope, that something grand has been done in America with the microscope, and that if 40 per cent. have been saved by using the microscope, it is not an "accursed" instrument. N. B.—These one hundred cases do not comprise all of my cases in consumption.

Rheumatism. Morph. A, E, C.—The microscope shows this to be a disease of the blood first, and next of the fibrous and cartilaginous tissues. Some call it a "gravel of the blood." Without the microscope no certain diagnosis can be made; with it the following varieties may be generally made out: 1. Cystinic. 2. Oxalic. 3. Phosphatic. 4. Uric. 5. Hippuric (rare). Besides these are enlarged and thickened fibrin filaments; thrombi, which may become emboli; adhesive and plastic red blood corpuscles, deprived of their covering of neurine, and thus clotting into firm ridges, rows and masses, like crowded and frightened sheep. These conditions usually are latent for a longer or shorter time, and are brought out by some secondary cause, as exposure to cold and wet. The gun is loaded before it can be fired off by the explosion of the cap. I have found the blood morphology of rheumatism

[1] See Clinical Morphologies—E. Cutter. They are too long to introduce here.

valuable in differentiating other diseases. For example, most people call fugitive and wandering pains rheumatism, and make out their own diagnosis for the doctor beforehand. But they do not know that other complaints will cause such pains also. So, when a woman complains thus, and does not have the morphology of rheumatic blood, the cause will often be found to be local. When a man complains thus without the morphology of rheumatic blood, the microscope has traced it to neurasthenia from catarrh of the urinary tract.

Another significant practical point found in rheumatism, is that there is generally an enlargement of the heart. The microscope shows that it is due to the adhesive condition of the red blood corpuscles, to the strong and more numerous fibrin filaments and skeins, to the blood crystals, and to the minute clots that are found in rheumatic blood.

When it is considered that the capillary circulation goes on in tubes $\frac{1}{1000}$ inch in diameter, it stands to reason that it would take more force to drive the blood with rheumatic morphology than when it is normal, with the red blood corpuscles clean-cut, distinct and covered with neurine so that they do not adhere to each other or the walls of the vascular system, and thus enter and pass through the capillaries with facility and ease. The increased force needed to drive the blood demands an increased amount of muscular tissue in the heart, just as the muscles of the blacksmith and athlete increase, and so comes the cardiac hypertrophy.

In such cases it is remarkable how readily the heart will resume its normal size when the morphology of the blood is restored to normality by treatment, and thus the microscope is a thing of beauty and utility in the handling of rheumatism and heart disease.

Pre-embolic State.—I have found the microscope to reveal the fibrinosis of embolism. For embolism is merely a plug of fibrin filaments or skeins stopping up an artery.

I have been able to avert this condition in childbirth by the use of the microscope.

Is not this worth some gratitude?

Blood diseases, such as leucocythæmia, malaria, erysipelas, boils, carbuncles; these I cannot understand without the use of the microscope.

Asthma and Hay Fever I find are explained by *Morph. B.* They are essentially a gravel of the lungs and nares. Epidemic influenza or infusorial catarrh is made to me a practical business matter by the use of the microscope. Bright's disease of the kidneys cannot be made out unless *Morph. E.* shows the casts of the kidney tubes and fatty epithelium, besides the macroscopic albumin. Neurasthenia in men is satisfactorily explained by the use of the microscope, showing catarrhal discharges in one of three forms: 1, Protoplasmic; 2, Skeins; 3, Indian-club shaped. Sometimes these forms are mingled together. A one inch objective is the best for this determination.

Diagnosis of cancer.—Diagnosis of healthy milk used as food. This is not easy.—Trichinous muscular tissues.—Eggs of intestinal worms in fæces.—Parasites, as itch insects, harvest mites, etc.—Diagnosis of ovarian cysts from fibro-cysts.—I find the morphology of the fæces gives indispensable information as to the food the patient has eaten; the character of his digestion, the amount of intestinal fermentation, the ability of the organs of digestion to do their work; shows the presence of tape and other worms, and is, with polarized light, a source of some of the finest microscopical exhibitions. Thus making the most repulsive and disagreeable objects in Nature æsthetically agreeable and attractive, certainly shows great transforming powers in the microscope. Reference is here made to the beautiful dissections of vegetable tissues found in the fæces. The same remark applies to the morphology of the sputum in a limited measure. Beauty is found in the ruins of the lungs.

The morphology of the air, which surgeons should study, has been very instructive to me. Malaria (or bad air) in *Morph. B.* has been a clear subject since this has been studied.

Morphology of dirt as found in and on the human body I have found very instructive in my business. Prof. Reinsch discovered vegetation on the surface of the coins used as money, and I have found the same in the dirt under the finger nails of my own hands. The late lamented Dr. Louis Elsberg coöperated in this study.

Morphology of Hydrant Drinking Waters.—Having studied the morphology of the hydrant water of over thirty cities and towns, I was called in court as an expert on a water privilege case. I was able to prove that the water was taken from the miller's supply, which had been denied. I am glad to know that the Massachusetts State Board of Health is paying attention to this subject of so great sanitary importance.

Morphology of food I find is a most interesting subject. It shows the changes produced by the various operations of cooking, which is connected with things which are supposed to be thoroughly understood because they come so closely in contact with the human body constantly ever since man came into the world. Foods are easily had, and easy to examine. Polarization shows how well the food is cooked, as starch and red muscular fibre do not polarize light when cooked, and of course when partly cooked, partly polarize light. Potatoes, beans, leavened and unleavened bread, crackers, dough, etc., are fine objects to study. There is no doubt that a great deal of sickness comes from imperfectly cooked food. I find the microscope an excellent means to tell what foods are bad for my patients, by studying the *Morphologies A, D, E.* Strawberries, for example, are poor food. This is a large, new and interesting field of study. I hope microscopists will cultivate this field more and bring out as good results as Dr. Taylor, of the U. S. Agricultural Department, has done with butters, true and false.

Morphologies of Invalids' and Infants' Food, Cereals Especially.—This division comes almost under the heading of adulteration of foods, so little do the advertised statements as to their character come up to their own demands. They are not adulterated, but wrongly described. I have tried to protect invalids and infants, with little success so far as I can learn. The microscopists who have this class of individuals in their families cannot afford not to examine their food, and protect the helpless ones from harm if not imposition. Of course this morphological examination should be supplemented by the chemical and physiological examinations. All have a right to examine their food as far as possible macroscopically, but the microscopical examination will acquaint the inquirer with the facts very much more satisfactorily.

Morphology of Adulteration in Foods is a large one

and has been made the subject of a volume by Hassall. There is great need that this examination be kept up, as these admixtures are so common everywhere. When microscopy becomes as common as music and is used as much, then the queens of our parlors will vie with the queens of our kitchens in securing pure, not "strictly," "absolutely" and fully "warranted" pure foods.

Morphology of Drugs in Powders, Extracts, Substance Crystals, Oils, etc.—I have not cultivated this department as much as I would like to, but I see here a large field for the use of the microscope as a means to the end of uniform and genuine preparations.

Morphology of Wearing Apparel.—I have found the use of the microscope to be of much avail in selecting silk patterns for dresses and clothing for my family. The time is coming when the microscope will detect the germs of diseases in infected clothing. It has come.

Some years ago the ladies of Boston held a fair to buy the old South church. A party in charge of one department perpetrated some frauds and erased as far as possible by friction the lead penciled items entered in the department memorandum book. I was able by low powers to reproduce nearly all the erasures, much to the delight of the ladies.

This syllabus, though incomplete, is enough to show that the demand of microscopy in pleasure and business is one that commands the respect of thoughtful minds, and should be extended to embrace thousands where it does now one individual, for it is an instrument of precision capable of great good to all.

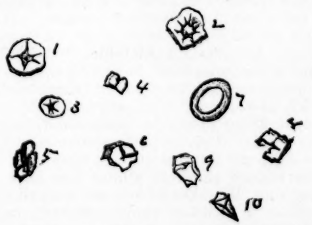
THE MORPHOLOGY OF RHEUMATIC BLOOD.

It is not intended to make this paper exhaustive, but to point out the prominent features. This corroborates the Salisbury paper in the American Journal Medical Sciences, Philadelphia, Oct., 1867.

Examination immediate, not mediate.—1. Collection. Specimens must be removed from blood stream as quickly as possible. The patient should be present and the blood should be examined directly, with no loss of time. This is particularly insisted on, as there is a general impression that blood can be collected by anyone and kept almost any time before examination. Mistakes have come from this misapprehension. 2. Kind of blood collected; capillary, not venous, nor arterial. 3. Site of collection.—The forearm, near the wrist. With some persons the fingertip is a favorite place for collecting blood, but the fat and dirt found there are objections, to say nothing of the needless pain of nerve papillæ puncture. If dirty, the forearm should be washed with soap and water, or ammonia and water, and rubbed dry with a clean towel previous to puncture. 4. Instrument for collecting the blood. Codman and Shurtleff, Boston, Mass., prepared the writer's scarificator, that can be buried to the depth desired with a single sudden painless motion. It is desirable to obtain no more nor no less blood than is sufficient to fill the space between an ordinary cover glass and slide. This amount is a drop about one-eighth of an inch in diameter. By thus preserving a uniformity in the size of the drop of blood as near as is possible, one can form an approximate judgment of the comparative number of corpuscles in different specimens, and the relative proportion of the serum to red and white corpuscles and of the red and white to each other. Best to clean the scarificator point by driving it into a clean towel after each use. 5. How to get the blood. Having the forearm and the instruments ready, the observer grasps the wrist with the left hand so that the skin is drawn tense; the scarificator in the right hand of the observer is applied to the radial or ulnar edge, whichever is held uppermost, avoiding veins, and the point is entered by approximating the thumb and fingers holding the scarificator. The point then pierces the skin so quickly as hardly to be felt, and if the capillary circulation is good, the cut fills with blood and exudes. Usually the sluggish circulation requires a squeezing of the cut to make the drop exude. A clean scalpel is then used to scrape off the blood and to transfer it to the slide, or the slide can be touched to the drop directly, when it is immediately covered and transferred to the stage of the microscope, previously prepared. 6. Power employed. Objectives, $\frac{1}{4}$ to $\frac{1}{8}$ inch; eyepieces, 2 or 1 inch. The writer uses Tolles'.

What to look for in rheumatic blood. 1. Red corpuscles, color, form, plasticity, adhesiveness. In rheumatism they are generally sticky and adhesive, outlines winged, huddling together in ridges or irregular masses like frightened sheep in a fold ready for shearing. They act as if their covering of neurine was removed, hence they travel with friction through the blood vessels and adhere together. 2. White corpuscles. They are apt to be too numerous, enlarged in size and filled with granules, which may or may not be salts in granular form, as the blood in rheumatism contains more saline bodies than normal. They also adhere to each other or foreign bodies and are found in thrombi and emboli. 3. Serum. After the blood has been a short time on the stage the serum will become filled with: (A). Fibrin filaments in network in the small meshes and strong threads, while in healthy blood the meshes are large and the threads so subtile and minute as almost to elude observation, like the outside hyaline carapace of a *Stephanoceros*. These same small meshes and strong-threaded fibrin filaments are seen in consumptive blood, and are due to a rheumatic complication. (B). Fibrin filaments in larger threads, ribbons and skeins. These are very marked at times in size and length. They oftentimes form skeins of considerable size and length. When long and narrow they will run through two or more fields of the microscope. Often they are of such transparency as to be almost overlooked, but usually they are of a white color, appearing like strings of fibrin in the blood, which they really are. When the red corpuscles crowd around them as crystals of sugar about a string in syrup, and a ridge is seen of unusual prominence, length, twists and curves, then a curl of fibrin filaments is usually found to be the basis of the configuration. Sometimes the skeins are inextricably coiled and curled up. Sometimes they are free like a lock of combed hair. Sometimes the skeins are very numerous, in which case embolism is to be feared. Sometimes they are few and isolated. Their unevenness and transparent snowy whiteness are usually enough to diagnosticate them from the mycelial filaments found in the blood, and which belong to the vegetable kingdom. They are found during pregnancy, and I have no doubt become a serious complication in some cases. Nature will clear them out of the blood if she has a chance to do it. (C). Fibrin filaments in thrombi that become emboli. These are found simple, when you can readily

trace the filament curled up on itself many times and retaining its cylindrical character. Or the mass is made up of the filaments with their bloody substance melted or fused together so that all trace of the filaments is lost, as in mother of vinegar, or embracing within their substance red and white corpuscles, crystals either entire or fractured, one or all. At times they are very large comparatively, and are probably formed by mechanical accumulation on the periphery of other fibrin filaments, or form elements encountered in the blood stream, as a snow ball grows by rolling in the snow. The presence of such bodies in the blood stream sufficiently explains the formation of fibrinous concretions on the valves of the heart or amongst its columnæ carneæ. Sometimes they are very large. For example, in the heart of the late Dr. Benjamin Cutter, the father of the writer, two fibrinous concretions were found each measuring eight inches in length. One originated among the columnæ carneæ of the left ventricle and extended into the aorta; the other arose among the columnæ carneæ of the right ventricle and extended into the pulmonary artery! It is easy to understand local swellings about joints and other parts of the body when such thrombi or clots become emboli or plugs in the adjacent blood vessels. When such are found in the blood of a patient there is need of immediate treatment, for sudden and otherwise mysterious deaths have been traced back to embolism. So if any proof of the value of the microscope in physical exploration was needed, this instance would serve and show that it is not a mere toy. This is not a paper on treatment, but it may be in order to here intimate that ammonia baths, and one pint of hot water flavored with the aromatic spirits of ammonia drank one hour before each meal and on retiring to bed, with medicines addressed to putting the liver, skin, kidneys and other emunctories in good condition, and regulation of diet will soon remove such thrombi, and no cure is thorough unless the microscope shows that the blood is restored to its normal morphology. Such is the positive value of the microscope in the treatment of rheumatism! (D). Crystalline bodies. These are generally, 1, cystine; 2, oxalate of lime; 3, phosphates; 4, uric acid; 5, hippuric acid, etc. These bodies are found in the human body normally in the urine and fæces, but usually in the blood when imperfect feeding or some other cause has deranged the functions of digestion, assimilation, organization and excretion. If the salts are found in too great quantity to be kept in solution by the eliminating fluids, they accumulate in those fluids. Uric acid is more soluble than oxalate of lime or cystine, hence it is natural that the latter are oftenest found in the blood. The phosphates of lime, soda and magnesia, accumulate likewise. The physical presence of these salts in the blood, fluids and tissues of the body, seems to be a sufficient cause of the pathological states we call rheumatism. The proof of this lies in the fact that when these crystals are removed, along with the other conditions named, a cure is effected. It is true that persons may have the morphology of rheumatic blood, and yet have no pains, no swellings, no troubles that they can detect, until they are upset by a cold or some other exciting cause. It will, then, be more accurate to refer the morphology of rheumatic blood to the domain of predisposing causes, it being comparable to the ammunition in a gun barrel, the cold or other exciting cause being like the pulling of the trigger. The explosion may be called rheumatism. If the gun is not loaded, or the trigger is not pulled, there is no explosion. So if the predisposing and the exciting causes are not present, we cannot have rheumatism. Once the morphology of rheumatic blood was found in a lady apparently in perfect health. She was surprised at the diagnosis, but coming later in contact with an exciting cause, she wrote, "You were right about my rheumatism;" and thus her case affords an instance of the value of the microscope in medical exploration. (E). Pigment matters. These striking objects are very impressive, and give the observer an idea that they come from the morphology of air, and hence are foreign bodies from outside the body. Giving all due allowance for this source of error, long study has convinced the writer that they, some of them, belong to the morphology of rheumatic blood. Their peculiar beauty of emerald green, bronze, yellow, black, in various shades, all invisible and unknown without the use of the microscope, opens up a new and lovely field of medical thought. What their chemical nature is, is unknown. This is a nice problem for solution. For our purpose, which is clinical medicine, it is enough for us to know their presence, and to get rid of them by putting Nature's means of elimination in good order, and giving her force enough to remove them. This, to us, seems the true way to practice medicine. 1. Cystine, $C_6 N H_6 O_4 S_2$ (Golding Bird). A crystalline substance found in the human body. It is the most common form of gravel found in rheumatic blood. Of course, in the blood stream the crystals are subject to friction, and cannot be expected to have the exact angles of the form found on the skin, or in the still fluid of the urine. In cut No. 1, one



was greenish inside and purplish outside; three was straw-color, four ultramarine, 5-10 greenish white. These were abundantly found in the blood of an adult man who suffered from acute sciatica. There were also found long skeins of fibrin filaments, strong fibrin network, red blood corpuscles, rather palish in color, sticky, and settling into ridges and huddled masses. Under a course of hot water drinks flavored with lemon juice, and with ammonia baths, along with alterative medicines, he was soon relieved of the sciatica, and his blood was found to be cleared of the cystine.

A patient in the practice of my son, Dr. John A. Cutter, sent from her home in Kentucky a specimen of urine which contained an unusually large amount of cystine crystals. He advised her that she was eating largely of the yolk of eggs, and that if she did not stop she would have rheumatism, and gave her some directions for treatment. She persisted in her dietary sins, and had what her local physicians called

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an extremely severe attack of neuralgia of the stomach.

Oxalic Rheumatism.—Oxalate of lime occurs in the blood, in amorphous granular masses. The urine is usually loaded with well-formed octahedral crystals, and may be milky in the granular oxalate of lime. They occur also in the fæces and expectoration ("gravel of the lungs"). Sometimes cystine is associated; sometimes other crystalline masses, crushed and fractured, are also found. Sometimes the oxalate of lime fills, more or less completely in crystals, a thrombus like plums in a pudding.

Lithic Rheumatism.—Uric acid and urates are found in the blood, in granular amorphous forms, rarely crystalline. Also in the urine, on the skin, in the fæces, saliva and sputum. Blood ropy, ridgy, adhesive. Emboli made up of fibrin and other formed elements of the blood.

Phosphatic Rheumatism.—Here the triple phosphates and the phosphates of the alkaline earths are found in the blood, in granular masses and crystals. Found in great rounded collections, which sometimes are beautifully colored an aniline blue of great purity. To make the diagnosis sure, they should be found also in the skin, or the urine and sputum. As the blood stands longer and longer on the slide and under the cover glass, these crystalline forms remain visible, with even better defined outlines than at first, So do cystine and oxalate of lime. The granules are not uniform in size.

Hippuric Rheumatism.—This is rare, and found usually in heavy feeders on vegetable foods, such as oatmeal. In the blood it is in granular form; crystalline forms in urine.

Carbonate of Lime Rheumatism.—Rare. Present in the blood in granular form, and in the urine in solid globes, with radial rays and dark color. Effervesce with acids.

Differential Diagnosis.—When persons complain of fugitive wandering pains, it is usually called rheumatism, especially if the pain increases at night; but these pains may be due to nervous prostration or neuralgia; that is, "pain in the nerves," the cause of which we are unable to find out. Once, at a medical college, I wished to study for demonstration rheumatic blood. A woman was brought to the dispensary with rheumatism. But her blood showed no signs of rheumatism. On examination, she was found to have uterine disease, which caused the pains.

Often have men come to be treated for rheumatism, and their blood been found to be normal, but careful exploration has found continuously in the urine, for the larger part of a week, protoplasm in skeins, or Indian club-shaped catarrhal discharge, which accounted for the pain by neurasthenia. A 1-inch objective is best for the detection of this catarrh.

THE DRINK HABIT.

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In discussing the above subject I start out with these propositions: 1, That any ailment which can be cured by moral suasion, religious enthusiasm or simple will power can not be organic in origin. 2, That the drink habit is curable by the above methods. 3, The drink habit is not primarily a disease of any organ. 4, That organic troubles are results and not causes of the habit.

There is much being written on alcoholism and there seems a universal idea that the habit is not merely habit, but is indeed and in truth a disease. Now, if alcoholism is a disease, how does the individual who has it get well without any treatment? We are told by the wise ones that alcoholism is hereditary; now, can any hereditary disease be cured without treatment? If so,which one. Of course there are many diseases that get well without treatment, but is that class of diseases known to be hereditary of this kind? Does any one know of a case of hereditary disease being cured by will power alone? Is there a physician, so far carried away by this comparatively new idea,that he will say many diseases,both organic and functional, are curable by will power? There is no use in putting the cart before the horse until it is shown that this is the better mode of travel. The profession have known all along that the excessive use of alcohol would bring about disease, but it is only recently that the advanced men tell us that the drink habit is a *disease* and that it ought to be treated as such, and when we find a patient with this trouble we should put him to bed and give him a regular course of medicine, diet him and nurse him until he is cured. Now for a case. I find Mr. A. drunk, and I learn from his friends that he is in a habit of getting drunk. We persuade him to go to bed. We give him such remedies as seem to be indicated and on the morrow he is all right and wants to get up and go about his business, but we tell him he has a very serious disease, it is hereditary and that it will take several weeks to cure him and that he must stay in bed, diet himself and take his medicine regularly, if he wants to be cured. Is is not plain to the unprejudiced mind that the above plan would not work, and that nine tenths of his (the doctor's) patients would pronounce him an ass. I'll tell you what. If you try this plan upon any man who has not injured himself, by the excessive use of alcohol, you will either get a cursing or lose a patron and perhaps both. But if you find some old bloat, who has spent his life sponging on his neighbors and who never had any manhood about him he will willingly acknowledge that he is a diseased man, he will go to the hospital and will accept of all the good things you will furnish him. He may be diseased, but it was brought about by exposure, hunger and starvation. Alcohol had but little to do with it; this bloat was vicious before he learned to drink and alcohol was only an incidental to his downfall and bad conduct; his association had as much to do with his condition as did alcohol. No one as yet, has told us that association is a disease, but there is as much reason in making such a statement as there is in saying the drink habit is a disease. I presume house-breaking and horse-stealing will ere long be included in the list of hereditary diseases. It is well enough to take care of diseased people, but once the people of these United States take upon themselves to house, feed and clothe the host of drunkards that are here and daily coming to this country then the pension business, with all its burdens of taxation, will be economical compared with the expense. No, sir, it is time to call a halt. This country is full of dead beats who are living at the expense of the working man, and it is a shame and a scandal for the medical profession to assist in fostering upon a credulous public the host of inebriates in this country. No, sir, inaugurate the whipping post, whip every man who is caught drunk

and you will soon find these so-called diseased men very well and hearty Mr. Keeley with his nostrum of gold would soon lose his job, and the host of drunken devils who infest cities and abuse and destroy all they can will soon be found at some useful employment. Punishment is the remedy for crime; God says so and I believe it. Fine old cure for a brute who is in the habit of beating his wife and children and living off of their wages, by putting him in a hospital and feeding and clothing him and telling him he is a diseased man. Don't any doctor with a thimbleful of brains know that drunkenness is a vice? Does any body excuse theft simply because the thief was brought up in bad surroundings? No. Neither should the drunkard be excused for his conduct. The fear of punishment is the only remedy that is effective with the vicious. Once set the precedent that crime committed when drunk is excusable and this government is gone, and if the profession succeeds in convincing the law makers, as they seem determined to do, that the drunkard is only a lunatic and not responsible for his acts, then you will see a scene of carnage that was never before witnessed in any land. Then for the sake of good government, for the sake of protected homes, for the sake of all that is good and for fear of all that is evil, stop this twaddle. Tell the law makers that you were joking and that you want every criminal pushed to the full extent of the law be he drunk or sober.

A METHOD OF CURING CHRONIC GONORRHŒA.

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The complete confirmation, in recent years, of Naegeroth's theory of the persistence of gonorrhœa in its chronic form, and its etiological relation to pelvic disease in women, has invested the subject of its cure with a new importance. Gleet is no longer a matter of inconvenience alone. The gonococcus in that bland discharge, or in the deeper and less noticed clap shred that is periodically washed away by the urine, is sufficiently virulent to establish in women any one of a train of evils, from the milder inflammations of the lower genito-urinary tract, to pyosalpinx with its periodical pelvic or eventually fatal peritonitis.

My own work in abdominal surgery contains the record of a number of these cases, fortunately cured by operation; but the great importance of the subject has been more forcibly illustrated by two fatal cases of puerperal peritonitis, caused by rupture of gonorrhœal pyosalpinx, that have come under my observation.

While making a microscopical examination of *tripperfäden* (clap strings), during some recent bacteriological studies in Berlin, I was impressed with the idea that *mechanical scraping must be combined with antiseptic irrigation, to cure chronic gonorrhœa in the male.* The pus cells containing gonococci of which these shreds are largely composed, are derived not alone from extravasated leucocytes, but more largely from embryonal cells thrown off by the infected granulation tissue in minute fissures and ulcers. Endoscopic examination reveals not only fissures and ulcers, but polypoid granulations projecting into the urethra. The microbic cause of the disease must be reached and destroyed in order to cure it. Caustic injections into the deep urethra undoubtedly often accomplish this, but the method that I have devised is theoretically and practically superior to any others that I know of for the cure of these cases.

It requires no argument now to prove the usefulness of the curette in curing infected granulating surfaces in sinuses, the uterine cavity, or any other situation in the body. Why the male urethra has so long been an exception, it is difficult to understand.

My original instrument was made for me by Thalen, of Berlin, and since my return I have had an opportunity of practically testing its value.

It may be called a deep urethral curette, and is so constructed that, while it will thoroughly scrape any portion of the urethral surface, it cannot possibly do any injury. It will be better understood by reference to the engraving than by description. There are fourteen holes, the upper or outer edge being sharp, and the lower or inner smooth, by the direction of the bore and further rounding of the lower edge. Each communicates in the interior with the one directly opposite, thus making it possible to readily and perfectly cleanse it. The position and arrangement of the holes has been studied with great care, and makers should not deviate from the model in constructing the instrument. It passes in as smoothly as an ordinary sound, and scrapes gently but effectually as it is removed. The treatment of obstinate cases of chronic gonorrhœa with this instrument should be proceeded with as follows:

As an essential preparatory treatment, the normal calibre of the urethra must be restored, if stricture exist, by the ordinary method of curing stricture. This is imperatively necessary, and treatment with the urethral curette must not be undertaken until a sound at least one size larger than the curette has been passed. Deep urethral injections of a ½ per cent. solution of chloride of zinc may be advantageously used for a few days before curetting. The urethra is then anæsthetized with cocaine, and the curette passed until the tip enters the bladder. It is then drawn back and forth in the prostatic membranous and deeper pendulous portions several times. The vicinity of the meatus may also be treated with this instrument, but more effectually by a simple spoon curette which I have adapted to this purpose.

After the curetting, antiseptic injections must be persistently used for a number of days in gradually diminishing strength. I first use a small injection of a 10-gr. solution of nitrate of silver with the deep

urethral syringe, then solutions of chloride of zinc, 3, 2 and 1 gr. to the oz., diminishing by a grain each day. I have succeeded in easily curing cases of years' duration by this method of treatment. The idea of thus scraping the urethra may seem savage, but as a matter of actual fact, it is not at all severe.

I bespeak for this method a careful trial by the profession, with the belief that it will mark a new era in the *cure* of this obstinate disease, now well known to be fraught with danger to innocent women.

I have sent my original instrument to Messrs. Chas. Truax, Greene & Co., who have duplicated it in two sizes, placing it, with the small spoon curette, the deep urethral and cone point syringe, in a convenient case.

Portland, Ore., April 1, 1892.

TWO CASES OF DYSENTERY, TREATED BY THE LONG CONTINUED USE OF BLUE MASS, WITH PERMANENT BENEFIT TO HEALTH RESULTING THEREFROM.

BY EDWARD ANDERSON, M.D.,
OF ROCKVILLE, MD.

During the summer of 1889, this section was visited by an epidemic of dysentery, with which the physicians here, myself among the number, were unable to cope. I tried everything recommended in works on practice, but was not satisfied with the result. I knew that my grandfather, a hundred years ago, used blue mass and opium in the treatment of this disease, but *how* to use it, I did *not know*. After repeated experiments, I found the proper thing to do was to combine ½ gr. of opium with 2 grs. of blue mass in a pill, and give one pill every two hours until the patient recovered.

On July 1, 1889, I was called upon to treat a lady, 56 years of age, all of whose family had dysentery, one son 20 years of age, having died a few days previously. She had had the disease ten days, and been treated by another physician with camphor and opium, but grew worse all the time. I fell heir to the case on account of the attendant's being attacked by the disease. I treated this case according to the above described plan for ten days, with gradual improvement, when I became appalled at the enormous amount of mercury taken, and stopped for awhile; but the patient immediately grew worse, and I recommenced the same treatment, and kept it up, lengthening the intervals between the doses as she improved, until October 1, when she was quite well. This lady's family history was good, every member except herself being robust, but *she* was thin enough to be remarked upon. Now she has gained flesh, looks vigorous, and one would scarcely recognize her for the same person.

The second case was that of a boy of 12, whom I had been treating several years for marasmus; he was attacked with dysentery last fall, and I treated him as I had the first case, taking about two weeks to complete the cure. Immediately after this attack, his health began to improve, and now he is one of the most robust boys in the neighborhood.

These were the only two cases that resisted this treatment for any length of time, all the others having recovered in from one to six days.

After the large experience I have had with dysentery, I do not believe it possible to salivate any one whilst suffering with the disease.

The most eminent pathologist in our State says, that the bacillus coli communis is always present in the human intestine, but is more abundant during an attack of dysentery, and that it is the dysentery germ. May not these two patients have been suffering all their lives from a superabundance of these germs, and only been freed from them by the protracted use of mercury?

A NEW SPIROMETER.

BY CHAS. DENISON, A.M., M.D.,
OF DENVER, COLORADO.

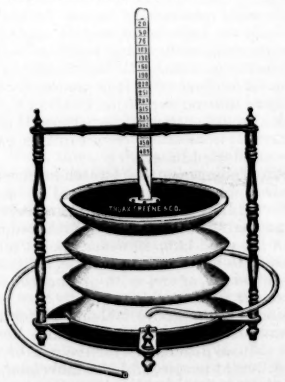
The superiority of this instrument, over any other spirometer in use, rests in its simplicity of construction. It does not depend upon water which will evaporate, or metal which will corrode, as is the case with the Hutchinson spirometer; nor does it require a certain amount of resistance to be overcome, as in elastic measurers. With Marsh's rubber spirometer this resistance is found by actual measurement to be equivalent to twenty millimeters of mercury, before any test of the expired breath can be recorded.

This new spirometer is capable of giving in cubic inches of air, the exact measurement of the *vital capacity* of the most delicate child or woman or of the strongest man. It is large enough to contain 6 small or 2 large breaths, so that an average record may thus be obtained if desired. If rightly made it is as nearly perfect as such a simple thing can be.

STANDARD VITAL CAPACITY TABLE.

Computed from 5,000 observations (Hutchinson) in healthy persons, standing, while making a full expiration into the spirometer, after a complete inspiration.

HEIGHT.	MALES.	FEMALES.	HEIGHT.	MALES.	FEMALES.
ft. in.	cu. in.	cu. in.	ft. in.	cu. in.	cu. in.
4 7	126	88	5 5	206	168
4 9	142	104	5 6	214	176
4 11	158	120	5 7	222	184
5 0	166	128	5 8	230	192
5 1	174	136	5 9	238	200
5 2	182	144	5 10	246	208
5 3	190	152	5 11	254	216
5 4	198	160	6 12	262	224



This illustration shows the simplicity of construction. The manometer, which is he re attached, should be used separately. A spirometer gives the vital

capacity, viz., the full number of cubic inches possible to be expelled from the lungs after a forced inspiration, while the manometer records the elastic tension which the air cells will stand under a forced expulsion of air from the chest, as compared with the average of a healthy man.

For one in health, the record at which the column of mercury can be held is about 100 M.M. of mercury.

This spirometer is purposely made to weigh very little, the light top of a cylinder, into which the air is breathed through the tube, being connected to the base by delicate impervious cloth in one piece. The size of this cylinder is so arranged that each rise (of the top) of one inch represents 100 cubic inches of of air contained therein, and this record is plainly shown by the movement of the recording rod attached to the top of the cylinder.

In the address of the President of the American Climatological Association in September, 1890, this spirometer was brought to notice in connection with a study of the different "air pressures within the chest, due to disease." On that occasion and since, I have demonstrated the lack of a just appreciation among physicians of the worth of the spirometer.

In my report on the tuberculin treatment of consumption, made to the Colorado State Medical Society in June, 1891, I formulated some rules by which a proper ventilation of lungs can be known to exist, in order that that mode of treatment may be successfully combined with the *high altitude* cure of consumption.

In brief, nature's method of attempting a cure of lung tuberculosis by the healing means of fibrous deposit, may and often does so shut up an effected lung as to preclude its proper ventilation, which is a necessary prerequisite to the use of "Koch's" lymph. While the patient's record with the manometer approximately shows his respiratory vigor, and the amount and strength of this new elastic tissue, it is the spirometric reading combined with the bilateral measurements of the chest, which will give the valuable information desired, viz., when and how far this process of shrinkage or hardening has been established.

If the normal spirometrical record for the patient's height is only one half reached, and the movements of the two sides are as one to two, then (without some compensation from the good lung) the ventilation of the affected lung must be about nil. It is practically as bad as though there were no expansion at all of the affected side, which would surely indicate that all of the one half reduced spirometrical record in this case, came from the unaffected lung.

By "without compensation" [which, however, is probably a false supposition], is assumed to mean that the unaffected lung does its full share as in health, viz., the one half of the normal. With compensatory help from the good lung, however, the affected lung could be said to be doing from a little, up to one third, of one lung's share, or one-sixth of what the two lungs together ought to do in health.

Assuming that the above table of Hutchinson's is correct, and making the little allowance needed for the slight natural differences for the use of the two lungs in different people, and the movements of the diaphragm in respiration, then it can be asserted with much accuracy that lesser variations in the above measurements, indicate proportionately less want of ventilation in the affected lung.

To obtain accurate bilateral measurements for comparison with the spirometrical record, a good way is to mark on the patient's chest, the middle line of the sternum, and then holding, or having some one else hold the steel tape measure against the spine of the opposite vertebra, use this line as a guide in determining the difference between a forced inspiration and complete expiration on the two sides.

The above rule is illustrated thiswise: If the spirometrical record of a given individual should be 260 cubic inches in health, and it is in fact one half that, because of a contracted lung, and the movement of the weakest lung is one-fourth as compared with three-fourths of an inch on the other side, the better of the two lungs is not only not doing just exactly its normal share, but it is probably compensating for the disabled lung, the work of the two, being somewhere between all on the good side (130 cubic inches), and three-fourths there and one-fourth of this 130 on the diseased side. If the spirometrical record (say 240 cubic inches) was nearly normal, as in some very favorable cases, and the bilateral movement bore the same relation to each other as above, then the compensatory work, the unaffected lung is doing for the disabled one, would be represented by fifty or more cubic inches, because that is the difference between the normal half, 130 cubic inches, and the three-fourths, 130 cubic inches, justly attributed to this lung.

Thus every inequality in these bilateral chest movements, when compared with the spirometrical record of an individual, is an indication of lessened lung movement, which should be explained by the physical diagnosis. The spirometer may not indicate the disease but its use is so important, that the physical examination must make clear the spirometrical record of a given patient, or that examination is untrustworthy. How important then that every physician, when examining a chronic lung case, should know what the vital capacity of his patient is, that he may be satisfied himself that his conclusions are correct, as well as that he may do his patient full justice.

Authors, by their lack of appreciation of the mechanical conditions within the chest made known by the spirometer, or by ignoring the subject altogether, have greatly injured a useful instrument, as well as the profession they would serve.

I have intrusted this instrument to Messrs. C. Truax & Co., of Chicago, to perfect and supply to the profession, and I have tried to protect them as far as possible in carrying out this work.

SYMPTOMATOLOGY AND TREATMENT OF SUMMER COMPLAINT.

Lecture delivered at the Fourth Special Course of the Chicago Policlinic.

BY W. S. CHRISTOPHER, M.D.,
PROFESSOR OF DISEASES OF CHILDREN, CHICAGO POLICLINIC.

The most prominent symptoms in summer complaint are those connected with the bowels. The diarrhœa is characterized by stools which differ from each other greatly, as to number, consistency, color, and odor. Of the several phases of the stools, the odor is the one to which I wish to call particular attention. It is indeed remarkable with what certainty the stools may be divided by their odor into putrid stools and acid stools. Occasionally the stool will be described as having an intensified fecal odor, or

smelling like old wood, but it is evident that such odors are but variations of putridity. The acid stools derive their odor from the presence in them of various members of the fatty acid group, all of which have arisen from fermentation of carbo-hydrates. The putrid stools, on the other hand, can only arise from fermentations of albuminous materials. I told you in my last lecture that we know of no poisons that can be produced by the fermentation of the carbo-hydrates, alcohol excepted, that can produce symptoms on the part of the nervous system, but that all the poisons that we know of, which are capable of producing symptoms through the medium of the central nervous system, and which may be formed from foods, are nitrogenous poisons. Let me recall some of the nitrogenous poisons: Every alkaloid which a vegetable contains is a nitrogenous product; morphine, quinine, atropine, the whole long list of vegetable alkaloids contain nitrogen; the long list of animal alkaloids, the ptomaines and leucomaines, contain nitrogen; every one of the toxalbumins contains nitrogen. The stools of putrid diarrhœa are liable to contain nitrogenous poisons. Why? Because putrid products can only occur from the fermentation of nitrogenous material. It does not follow because a stool is putrid that it contains poisons, but it shows that it has been formed by the fermentation of proteids and therefore probably contains poisons. If, on the other hand, it is sour, it shows that fermentation has been maintained by the carbohydrates, from which such poisons cannot be elaborated. Baginsky says that one stool may be sour and the next one putrid, the next one sour, and so on, from the various forms of fermentation that may go on in different parts of the intestinal canal at the same time. Theoretically this is true but practically it is not. When we find a child passing putrid stools it continues to pass putrid stools for several days, and when it passes sour stools it continues to pass sour stools for several days. The acid form is a milder form of the trouble, it produces agents which act locally only, whereas the putrid form produces agents which act through the central nervous system, therefore if we are able to transform a putrid diarrhœa into an acid diarrhœa we eliminate certain definite sources of danger. We may have a putrid and a sour fermentation going on at the same time, the sour fermentation being masked by the more pungent odor of the putrid fermentation. But the putrid fermentation is an indication of danger. My friend Dr. Rachford of Newport, Ky., has called attention to the fact, that if we have fermentation of proteids with the formation of poisons, whether there happens to be putridity or not, there will be symptoms referable to the central nervous system, and therefore if we have such symptoms due to poisons formed in the intestinal canal, whether with or without putrid stools, we should treat the case in the same manner as I shall teach you to treat putrid stools.

We may have stools which are large and watery, we may have stools which are small and more or less coherent. The stools may be characterized by containing blood and mucus, they may be accompanied in their passage by pain, or by straining, or by bearing down, tenesmus; they may be characterized in color by being pale or yellow or green or brown, but every one of these changes is referable to the variations in the fermentations in the bowel. It seems certain that the green stools of summer complaint are due to certain chromogenic bacteria, but the green stools are by no means the worst.

In many cases where the stools are green, due to the growth of certain chromogenic bacteria, we have no general systemic trouble, but on the other hand, where we have rather innocent looking stools, nothing but a little rice water discharge, our patient is seriously ill. In every such case of cholera infantum there has always been putrid stools at first, and they are no longer putrid, simply because everything has been washed out of the bowel. My own experience is, that brown stools are the most foul in point of odor, and the most apt to be accompanied by severe symptoms on the part of the general nervous system. The variations in color are unquestionably due to the variations in growth of the microörganisms in the intestinal canal, but may sometimes be due to the food. Mellin's food, for instance, produces a brown stool, but such stools are innocent. Some of these patients have fever and some have not. Why? Simply because in some instances poisons capable of producing rise in temperature are formed, in other instances poisons of this kind are not formed, so in one case we have fever and in another we do not. Fever is then purely an accidental condition. Again, we will have on the part of the brain such symptoms as convulsions. But convulsions are not due, I take it, to simply the draining away of the serum which the large number of movements has produced, but to the production of a convulsive poison, something that can produce convulsions, something like strychnia. I do not say that strychnia is elaborated in the bowel, but a poison having a somewhat similar action. Another point of importance is the question of coma in these cases. Some years ago, when I treated these cases exclusively by opium, I used occasionally to find a little one with what I considered opium poisoning. Later, after I had stopped using opium, once in a while the same kind of case would occur. On questioning the mother I would find that no opium had been given outside of my direction, and something else must be at fault. I see several such cases every year, characterized by contracted pupils, stupor, sometimes coma, by slow respiration and slow pulse. Now there has been formed in the bowel of that child a poison which acts like opium and produces these symptoms. How shall we get rid of it? I have invariably given the nitrate of potash and water, and in the course of two hours, invariably profuse urination has occurred and with it all symptoms have disappeared; in other words, the poison has been washed out through the kidneys. Here, then, is one set of symptoms which is evidently produced, so far as clinical information can teach us, from something developed in the bowel. Again we have patients troubled with insomnia, and it is likely that this sleeplessness is caused by poisons produced in the same general way. Now, let us take up the question of depression, for that is the most important one. Our little patient is weak, the fontanelle has sunken, the eyes have sunken, the skin is cold and the pulse indicates collapse. Now to what is that due? Is it because the baby has had four or five or a dozen stools that day? No. If you had given the baby sulphate of magnesia there would be the same loss of serum. Would the child be in that shape? No. There is another element there besides the number of stools. I believe the mere number of stools is of secondary importance. The question of loss

of fluids we have been taught to think much of—too much indeed. But let us suppose we had introduced a motor depressant into this child's blood, say conium or curare, what would have happened? Almost the same condition of collapse. It is evident that the collapse has been produced by some poison, and not simply by the number of stools that have been passed. If we believe the collapse to be due wholly to the number of stools we would most rationally proceed to stop the stools; we would give the child opium, tannic acid or anything to stop the flow from the bowels. But if it is due to a poison that has been formed in the intestine, what are we to gain by stopping the number of stools? Nothing. We may even do damage by retaining in the bowel the source of the trouble. I beg of you, then, to regard summer complaint as presenting various phases, but due entirely to poisons formed in the intestinal canal. On the other hand, we find that sour stools cannot produce poisons of this kind, but still we have movements from the bowels, and we can only explain the phenomenon on the ground that the products of acid fermentation are directly irritating to the mucous membrane of the bowel and act by locally stimulating the bowel, by locally increasing its functions. But remember that such irritations do not and cannot produce collapse; that they do not and cannot produce coma; that they do not and can not produce convulsions; all the serious conditions are thrown to one side and only the local ones of increase in the number of stools and pain remain. Now then, here comes in the explanation of Dr. Rachford. If these depressed conditions occur, no matter what the number of the stools may be, the trouble is due to poisons formed from nitrogenous material. If we can stop the proteid fermentation, even if we have to leave an acid fermentation, we shall make a decided gain for our patient.

Now as to the treatment proper. It consists of two general steps: first to remove the cause, second, to repair the damage done. These two steps apply to almost any disease, but particularly to this one.

First, remove the cause. How? We have found that the cause is due to certain fermentations going on in the bowel; let us get the poisons out of the bowel. How? by the use of laxatives first. To empty the bowel of a baby, gentlemen, is a work of art, it is not a simple thing. We can give a dose of castor oil and have a certain number of stools produced; we can give a dose of calomel and have a certain number of stools produced, but we are by no means certain that we have emptied the bowel of the child. Don't forget this point; it is exceedingly important. I shall refer to it again. My preference by way of preliminary cathartic is calomel in grain doses four hours apart until three grains are taken. I give a child six months old one grain of calomel every four hours until it has taken three doses. Sometimes a child may be having eight or ten stools a day; you give it calomel and it will be brought back the next day with a report of four or five movements instead of the increase you had reason to expect. I cannot explain this phenomenon, but it occurs with sufficient frequency to attract attention. The actual number of stools will be diminished under the action of the cathartic, and what is more, the child will be better, irrespective of the number of stools. Sometimes the number of movements will be very much increased; the child will be having four or five a day and after taking the calomel it will have eight or ten or more and yet be better. I have never seen any bad results from the use of calomel, although bad results are reported under such circumstances, but I have prescribed it so many times and given it to so many babies under these circumstances that I have no hesitation in giving it in the quantity mentioned to a child six months or older; the quantity need not be increased for older children. If too large a dose is given it is simply swept out of the system, there is no such thing as giving too much in order to purge freely. But understand, that the calomel empties the bowel only partially, and by no means does it always get everything out. What shall we do next? To still further empty the bowel I would recommend the use of copious injections of water, and they can be introduced either with or without the aid of a rectal tube. The quantity of water to be put into a child's bowel is something to be considered. In one method of irrigation, the water is allowed to run in and out at the same time. By that means only the lower part of the bowel can be washed. My plan is to fill the bowel with water, I want to reach the cæcum, because there is where most of the trouble lies, where the greatest amount of damage has been done, where the autopsies prove that most of the poisons have been absorbed. Because there is where we have the most ulcers, and no matter whether we have enteric disease or dysenteric disease symptomatically, the actual lesion is in that part of the colon. I put into a child a year old a quart of water at a time, and it will hold it. This seems rather a large quantity, but patient after patient I have had in my clinic, brought before the class for the distinct purpose of showing them how much water a child will hold, and it is remarkable how much they will take. I set the quart as a limit; a half gallon will nearly fill an adult's colon. But some judgment must be exercised with regard to the quantity used; if you have reason to believe that the bowel is badly ulcerated and liable to be torn because of pressure, you should not put in such a quantity of water, but in an ordinary acute case you can put a quart in. A fountain syringe is the safest means of introducing the water. When it is in it runs out again, of course, as soon as you take away the tube. But the bowel does not completely empty itself at once. The mother will tell you that the child passes about a pint and then stops, and the remaining water will come away in about twenty minutes or half an hour. Do not put into the water you use to wash the bowels, any poisonous antiseptic; do not use bichloride of mercury to wash the bowels, because you might have enough of it stay behind to produce poisonous symptoms. It is not needed; clean water is quite enough. The object of the water is to wash out the bowel and clean out the little loculi. You remember in the large bowel there are little pockets at one side which form convenient places for fecal matter to lodge in, and when we give a brisk cathartic like calomel, it strikes right past these pockets and does not clean them out; but with the slow washing out of the bowel the water will seek out these pockets, and clear out their contents. Very frequently these masses of retained feces are the sources of the poisons. This illustrates the substantial identity of putrefactive constipation and summer diarrhœa, and justifies their being included under a single term.

Now we have gotten this bowel clean, apparently

so at least, by means of the preliminary purge and the washing. So far so good, but we have only taken out of the bowel the larger mass of material there present, we have not washed it clean. You could not sterilize in that way a test tube, and much less so complicated a tract as the bowel. You have not got it clean but you have got the bulk of the material out and you have removed the bulk of the poison, but the germs remain behind, at least enough of them to set up the trouble again. Our baby will probably seem better, some of the symptoms have disappeared and the child is undoubtedly better, but we want to kill the germs there, we want to kill the particular germs that are producing the trouble. How can we do it? We have at our disposal a large number of intestinal antiseptics, the best among them are unquestionably calomel and bismuth. The calomel which we have originally given acts as an antiseptic, but its action is only temporary, it does not and can not sterilize the bowel. If we wish to use it still further as an antiseptic we will give minute doses of one-tenth or one-twelfth of a grain every three or four hours, but the results will be only partially satisfactory. Next to that our best antiseptic is bismuth and to employ it successfully as an antiseptic we must use it in massive doses. The dose of bismuth, like calomel, is not to be measured with great accuracy. I usually give a dose of ten to fifteen grains three or four times a day to a child six months or a year old. Bismuth does not produce any systemic effects, but you want to give enough to produce an effect in the intestinal canal, and two or three grains of bismuth are not enough. There is one objection to bismuth. Besides being an antiseptic, it unquestionably is, to a certain extent, an astringent. Many times I have thought the diminished number of movements following the use of bismuth were due to the curative effects of the drug and that the poison had been destroyed, but I have reason to believe that it was really due to the astringent effect of the bismuth, and this effect in the early stage of the treatment is not desirable because it retains the poisons we want to get out. Then we have naphthalin, salol, resorcin and a whole host of drugs of that type, but they are unsatisfactory. Fortunately we have a much better means at our disposal. Suppose we have a diarrhœa and the stools are distinctly putrid; we know there has been fermentation and that the germs producing it are living on proteid material, so we simply keep that kind of material out of the bowel and starve the germs; there you have the keynote of the whole method of feeding. Starve the germs, do not try to get them out with chemicals because you cannot do it. You accomplish something in that way but you cannot kill them all. But when the child has a putrid diarrhœa you must keep away from it all proteid material, keep away meat, fish, milk and eggs. Keep away those foods which are capable of undergoing putrefaction. Meat would not be given to a child of that age; fish would not be given to a child of that age; eggs would possibly be given, but milk would most certainly be given. Milk has been the chief diet of this child before it was taken sick, and the proteids of milk are what the germs in that intestinal canal are best adapted to live on. Above all things stop milk; that is the first thing to do when you have a putrid diarrhœa. Do not be led into giving milk because the books tell you that it is a bland and non-irritating diet. That means nothing. We are not trying to save that bowel because it is in an irritated condition, but we are trying to prevent the formation of poisons, and therefore we will keep out of the bowel material from which they can be formed, and that is in this instance proteid material. Now if we have to keep proteid material out what shall we put in? Anything which cannot support the obnoxious germs; anything which will starve them out and still be food for the child. The books tell us that in severe cases we should stop all food. If we stop all food we can certainly starve the germs but it strikes me we are going to starve our little patient also, and put a strain upon him that is unnecessary. If we must take away meat, fish, eggs and milk let us give him the starches and sugar. Many years ago Moore, of England, advised that these cases be fed on cane sugar exclusively. Such a diet is right, but you don't need to limit them to cane sugar; give them starches. I take a child six months old with putrid diarrhœa and give it arrow root, or rice, or crackers, or baked potato, but not milk. But you look surprised to hear me advocate potatoes and crackers — solid food in diarrhœa. When we have the theory of this disease that I have attempted to give you, that it is not due simply to an irritated condition of the bowel, why not give solid food? I assure you, from a personal experience of several years in this matter, that solid foods act most beneficially. Milk is a solid food; it is not liquid. It is only liquid before ingestion. The first thing that happens to it in the stomach is coagulation, so that it is probably more irritating from a mucous membrane standpoint than any amount of starch. We are told these babies cannot digest starch because they have no salivary secretion and no distinct pancreatic secretion. I will admit the physiological statement that their salivary and pancreatic secretions are both deficient, but I deny the other statement. It has been assumed that because these two secretions are deficient, that infants cannot digest starch; but we all know of babies who have been given arrow root from their birth, and who have digested it perfectly. We all know that babies can and do digest starch; they do it daily. Why, there is hardly a secretion in the body but is capable of digesting starch so that it can be absorbed. Right here I wish to be understood that I do not advocate starch as a continuous food for infants, but I am speaking of its use for temporary purposes; it does help that child along when it is poisoned; it does serve a purpose with that child when we want to take away other kinds of food, and in giving starch we have the advantage of not depriving that little one of all food. How about predigesting the starch? We do not need to predigest the starch; the great bulk of children can digest starch, particularly the small amount necessary for our purpose.

I told you that milk was bad for this condition; how about peptonized milk? Peptonized milk is worse. Why? I know some of you have used peptonized milk, and in some cases with good results, but I wish to assert that if you did get good results in those cases, you could have gotten better results in some other way. This preparation is generally bad. In the first place, it is peptonized by means of a pancreatic ferment. If you will experiment, by putting some hard boiled egg in a solution of pepsin and hydrochloric acid, and leaving it over night at the temperature of the body in a suitable oven, you will find that it has not only digested completely, but

that it has a peculiar odor that is not unpleasant; but take the coagulated egg albumin and put it with trypsin in an alkaline solution, and subject it to the same conditions, and you will find, as you open the incubator, that it has a distinct fecal odor. If you leave it a few hours longer, the fecal odor will become stronger, and in a few more hours the odor will be unbearable. The mass has become putrid. Now the action of trypsin upon proteid material is to break it up in such a way that the germs of putrefaction can thrive therein. They thrive readily in the products of tryptic digestion, but not readily in the products of peptic digestion. The milk partially peptonized by this pancreatic extract, is only in a better shape to undergo putrefactive changes when it reaches the stomach and bowels. If you put it into a perfectly healthy alimentary tract, no harm will occur; but if you put it into a bowel that is already contaminated with putrefaction-producing microörganisms, you have only helped those microörganisms to the extent to which you have digested that food. Understand, I do not say that there are no conditions in which peptonized milk may prove useful, but am merely condemning its use in the putrid diarrhœa of infants.

Now about sterilized milk. It is practically pure so far as germs are concerned, but just as soon as the sterilized milk reaches the infected intestine, the germs there begin to develop in it; not so readily, perhaps, as in unboiled milk, but just as certainly as in any other kind of milk. Sterilized milk, therefore, is not the proper food in putrefactive diarrhœa, because it has essentially the same chemical composition as any other milk, and it is the chemical composition that is guiding us at present, and not its bacteriological condition. Sterilized milk, therefore, should be used solely as a preventive; if we start with a healthy child and feed it sterilized milk, we run that much less chance of introducing pathogenetic microörganisms into the intestines, but after they are once there, it has no power to get them out. Milk from the mother's breast does just as much damage as milk from any other source. Other things being equal, I would rather treat an acute case of diarrhœa in a bottle baby than in a breast baby, because I can give the bottle baby just what I want to. Frequently I would take the baby from the breast for thirty-six hours, keeping up the mother's secretions by the breast pump, and feed the baby as I would like to; but we cannot always do this, and where I cannot do it I find that I do not get as good results in treating diarrhœa in breast fed babies as in bottle fed babies; I cannot cure them so quickly because I cannot feed them right. Such a procedure is only necessary in severe cases.

How long is it necessary to withhold proteid food in acute diarrhœa? Twenty-four hours will usually suffice to correct the odor of the stools; at the end of that time stools which have been rotten will have lost their putrid odor, usually, and at the end of forty-eight hours they will certainly have lost their putridity. But it is desirable, where possible, to withhold proteid food for several days after the stools have lost their putridity. In chronic cases, where there is already ulceration of the intestine, the putridity is maintained by something besides the intestinal contents. Let us say the putridity has ceased, what will occur? Almost always the stool will become sour; in other words, an acid fermentation which has been going on in addition to the putrid fermentation continues, so that we still have left a cause for diarrhœa, but we have transformed a diarrhœa which was capable of causing nervous symptoms into one which is only capable of producing local symptoms in the bowel; we have transformed a dangerous trouble into one comparatively simple. That is what has been gained by the method of feeding.

I think this will give you, in a general way, my idea of how to remove the cause. Remove it by purges, remove it by washing out the bowel, remove it by antiseptics, and above all by starving out the germs by withholding their proper food.

One other thing: that is washing out the stomach. It frequently happens that we cannot by all these means at our disposal control things as we would like to, and it becomes necessary to wash the child's stomach. Washing the stomach, particularly where vomiting is present, is of great service, but you are not required to do it very often. I have had comparatively few cases in which it was absolutely necessary, but I believe if I had resorted to it more frequently I would have had better results, would have cut short more cases. But it is a very troublesome measure. Washing out the bowel is an easy matter, and can be done by the mother, but washing out the stomach is something you must do yourself.

The question of the treatment of vomiting in these cases is to be considered. The vomiting is probably not due, not always at least, to local conditions, but most likely it is due to the poisons produced by the various fermentations present, and it disappears when the poisons are removed. When it is present, however, it is a very serious complication, as it prevents us putting anything at all into the child's stomach. If persistent vomiting is present, we should always wash the stomach by means of the stomach tube, and this often gives relief.

We have removed the cause, how shall we repair the damage done? Ordinarily, with the removal of the cause, the child promptly recovers. But the matter is different in a severe case where the child has been seriously ill from a sharp attack of cholera infantum, and at the end of a few hours of illness is in a state of collapse; sunken eyes, sunken fontanelle and pale, cold surface; rolling the eyes about, opening the mouth, showing the dryness of the lips, etc. What shall we do? Evidently, in such a case, there is no time for delay, the damage is too severe, the poison must be directly combated. We will of course stop all food at once, because we cannot run the risk of doing that child further harm; particularly would we stop milk. We would wash the bowel as far as we could, for a double purpose, the one to remove from the bowel as much offending material as possible, although but little will be found; the other, to supply some of the fluids which have been lost. The water used should be as warm as can be borne. The peculiar depression must be combated by all possible means. First, by external heat in the shape of hot baths and hot blankets wrapped about the child to stimulate it. Opium here is of great value; so also is belladonna; it directly counteracts the poison present, it is a stimulant to this particular condition. Another thing: if it is possible to get fluid of any kind into the child, do it. Why? Because that fluid will pass out again through the bowel, possibly through the kidneys, and as it goes

it will wash out some of the poison which is doing the harm. Our object in a case like this is simply to support life. We cannot attack the case on the lines I have given you before; we must counteract the poisons as well as we can by heat, belladonna, opium in small quantities and by washing out as far as we can through the process of elimination.

In chronic cases, those which have lasted three weeks or more, the greatest good is accomplished by regular daily lavage of the bowels. The use of intestinal antiseptics, particularly bismuth, is of considerable service. In long-continued cases, laxatives can only be used occasionally. While the diet is to be directed on the same lines as those already laid down, it need not be adhered to so absolutely.

We all have our type cases of which we make much. Among mine is one which I should like to relate to you now: A rachitic baby, which had been under my observation for regulation of diet, was suddenly taken with convulsions. Apparently the bowels were in a pretty fair state at the time. The convulsions commenced at 4 o'clock in the morning, and lasted until noon. I first saw the child about 8 A.M. The temperature then was 102° in the rectum. I suspected the bowel as the source of the trouble, and endeavored to empty it. I gave the child calomel by the mouth, washed the large intestine, and gave by the rectum a small dose of croton oil in emulsion. Quite free purgation ensued, but the convulsions continued, and I was forced to think that they had their origin in some other cause, most likely the onset of some infectious disease. I was finally reduced to the necessity of administering chloroform to control the convulsions. In the afternoon the convulsions returned, and the temperature rose very rapidly until it reached 107.½°. Cold baths and antipyrin were employed without success in combating the temperature. What was making this temperature? Just before the little one died, it passed a mass of the most putrid feces I ever smelled. There was the source of the trouble; it was absorbing poison from this mass, and if I had continued trying to empty the bowel, possibly I could have saved that child.

Now then, to repair the damage done, we must meet the symptoms as they come up. If the child has fever we will endeavor to meet the temperature by sustaining the child's strength with alcohol and by producing sweating. If the temperature be high you can produce sweating in several ways. One way is to sponge the body over with almost ice-cold water, then wrap the child up well; there is an immediate reaction. The skin becomes red and soon there is profuse sweating. That can be done with a baby where you could not do it with an adult. So we take the temperature down, as in any other disease, but particularly do we look to the bowels as the source of the trouble in these cases. In those cases where the temperature runs up to 106° and 107°, and which are sometimes called sunstroke, the trouble is probably always in the bowels, and you should remember that even after you have given a cathartic you are not through with the bowels until the symptoms cease.

I should like to say a word regarding the opium treatment of this disease. For a number of years I treated these cases with opium. There can be no question but that good results can be obtained many times by the use of opium. In the first place because of its stimulant effects, and in the next place because it relieves the pain and thereby strengthens the child. If used in the shape of a Dover's powder, it acts as an eliminant and we have some of the poisons thrown out, and in general it is a direct antidote to the poison, so that the use of opium amounts to treating the poison by a direct antidote instead of treating the disease by getting rid of the poison. It is not so illogical as one would imagine, but I don't think the treatment is good; relapses, so-called, are more apt to follow the opium treatment than the plan which I have outlined.

Another thing please do not forget; that is, that you are at all times treating the baby, and not the disease. Never treat a case of putrid stools, but be kind enough to treat the baby who is unfortunate enough to have those putrid stools. We see cases where the child is passing the most abominable stools, and yet it may be well, happy and lively; don't put such cases amongst those I have asked you to treat; they are going to do well enough if you will let them alone. We must use judgment in these things. We have many cases come to us that fortunately do not present the exact conditions for interference on the lines I have laid down for you; and remember they do not need any such interference; they will get well if you wash out the bowels. You must remember that it is not simply the putridity that directs you to interfere, but the continued severe nervous symptoms accompanying it.

Suppose the stools are already sour; theoretically, you have got to keep out the starches, but what have you to fall back upon? Nothing in the world but white of egg. At one time I tried this plan, with the result that I succeeded in changing acid diarrhœas to putrid diarrhœas. A sour diarrhœa we can treat with bismuth and complete withholding of food, or the use of milk. The milk will keep up the sour diarrhœa because of the sugar in the milk, but it is the best we can do. Remember that a sour diarrhœa is not ordinarily dangerous. We can also give opium, and this is the place where we may want opium. The acid produces colic in the bowel and also flatulence. The gas does not cause the pain; it is the acid that causes the pain. Lavage of the bowel can also be profitably employed. The acid form of the trouble is not so amenable to dietetic treatment on the plan I have given as is the putrid form, and we must treat it on other grounds.

SOCIETY PROCEEDINGS.

Chicago Pathological Society.

Regular Meeting, April 11, 1892.

DR. C. D. WESCOTT IN THE CHAIR.

Dr. Edward F. Wells read a paper entitled

PNEUMONIC GANGRENE.

He said that Laennec, to whom we owe the first clear and precise treatise on pulmonary gangrene, met with only a few cases in all his immense experience. Andral, with unsurpassed facilities for observation, had no well defined views of his own and contented himself by saying that the very existence of this termination had been denied by some, and that in his opinion the entire subject required further study. Stokes never met with a case in childhood, but saw several in adults. Copeland observed the condition but rarely. Cullen states that the termination of pneumonic fever in gangrene is much more rare than has been imagined. Trous-

seau, in the course of an experience, which for extent, breadth and duration, is accorded to but few, never saw a case. Elliottson also never met with an example. Walshe says that it is "one of the very rarest terminations" of this disease, and similar words are used by Frank, Lépine, Tanner and many others. Of Grisolle's three hundred and five cases not one terminated in gangrene. Sturges goes so far as to deny that gangrene is ever a consequence of the pneumonic process. Ziemssen met with it only rarely, and Baginsky not at all in infantile cases, whilst West does not consider it very common.

Were every case of pneumonic fever accompanied by fœtid breath considered as one of pulmonary gangrene we would meet with this complication on every side and in abundance. If, however, it be required that the diagnosis be established by a sufficient array of corroborating, if not unequivocal signs and symptoms in the cases ending in recovery, and by the indubitable evidence of *post mortem* section in those terminating fatally, they will be encountered much less frequently.

But even the evidence of the dead house, as reported in times past, must be accepted with caution. Thus the ancients speak freely and often of a gangrenous condition of the lungs, but it is more than doubtful whether they refer to an actual death of the tissues, to which state we now limit this term, or merely to a dark or black color of the parts. That this inferential unreliability of their accounts is well founded is evidenced by the consideration that, although a gangrenous condition is so frequently referred to, yet the leading and obvious symptom, that of fœtor, which could scarcely escape notice, is not often mentioned in this connection. Even Graves, only half a century ago, spoke of lung tissue breaking down easily between the fingers, "resembling gangrenous lung, except that there was an absence of fœtor."

Gangrene of the lungs may arise from one or more of several conditions. Thus a cavity—tubercular, from abscess or from dilated bronchi—within the hepatized area may be the starting-point of the necrosis. The stagnant contents of the cavity putrefy and infect the cicatricial walls, which from the nature of their blood-supply are illy able to withstand an energetic inflammatory onslaught.

Cases of this kind are by no means rare. Laurent reports the case of a consumptive lady, who had had several attacks of hæmoptysis, who was taken with an acute pain in the right side, followed by the ordinary symptoms of pneumonic fever. On the fourth day the cough was violently convulsive and she expectorated a large quantity of greenish pus, mixed with a black substance having a horribly fœtid odor. Expectoration continued for ten months when recovery finally ensued.

Two of Dr. Wells cases were of this nature.

Case 1.—A farmer, aged 60, temperate, but with a broken-down constitution, and long a subject of chronic bronchitis and well marked bronchiectasis, was taken, Oct. 20, 1878, with the ordinary symptoms of pneumonic fever, locally affecting the upper lobe of the left lung. On the eighth day, and suddenly, the breath and expectoration became horribly fœtid. The cough was infrequent, not very severe, and the comparatively scanty expectoration was raised without much effort. The expectoration was of a dirty, reddish-brown color, thin and not viscid. After standing for twenty-four hours it exhaled a valerianic odor which was not particularly unpleasant. If, however, the fluid was agitated the fœtor was again manifested. At the bottom of the conical glass there was a considerable sediment, consisting of pus cells, altered blood corpuscles, etc. Elastic tissue was not recognized. The patient at once passed into a state of extreme prostration, with coldness of the surface, sub-normal temperature, sighing respiration, apathy, etc., and so remained for several days. On the sixteenth day of his illness he was able to sit up and with slight support to walk about the room. The quantity and quality of the expectoration varied from time to time, but it gradually became more purulent and less fœtid, so that at the end of six weeks it was no longer gangrenous. The patient remained weak and unable to work for several months, but he eventually entirely recovered.

Case 2.—An intemperate farmer, aged 42, had an attack of pneumonic fever in November, 1874. The exudation material was slowly removed, his cough continued, and in the June following he had hæmoptysis. The case developed into one of unequivocal phthisis, with the formation of cavities. In December, 1880, after exposure to cold and wet, he again suffered an attack of pneumonic fever, locally affecting the left lung. By the sixth day the fever had disappeared and convalescence seemed assured, but during the night symptoms of collapse set in and he died, suddenly, early in the morning. At the autopsy the upper lobes of both lungs were found riddled with tubercular cavities. The base of the left lung was found hepatized, mottled on section, and the bronchi filled with muco-purulent matter. In the upper portion of the lower lobe, near the root of the lung, posteriorly, was an abscess cavity, the size of a pigeon's egg, with thick walls, and a free opening into a large bronchial tube. It was full of pus and a portion of the wall the size of a finger-nail was dark, shaggy and gangrenous. This portion readily broke down under pressure and exhaled a very fœtid odor, which clung tenaciously to the fingers.

Gangrene may result from excessive exudation into the alveoli and pulmonary parenchyma. In these cases the circulation is so much impeded that death of the parts ensues. This is probably the most frequent cause. It has been supposed that a highly hæmorrhagic exudate is especially prone to be followed by gangrene, but this is scarcely possible. Delayed absorption of the pneumonic deposit and gangrene frequently appear together, because they are both due to the same cause—insufficient blood-supply.

Pneumonic gangrene is no respector of sex, and is observed in the two sexes in about the same proportion as pneumonic fever, viz., 73 per cent. of males to 27 per cent. of females.

Thus the cases of Huss—twelve—were all males. Of the nine cases observed by Kaülich 7 were males. Of Lebert's 32 cases, 22 were males, and of Dr. Wells' 8 cases, 7 were males.

It has been thought that gangrene is more frequently the issue of pneumonic fever in the aged than at other periods of life, but the information at Dr. Wells' disposal leads him to conclude that age, beyond that of childhood, is not an influential factor.

Thus of 60 cases analyzed by Lebert 10 were under 20 years of age, 37 were from 20 to 60, and 13 were over 60. Of 63 cases analyzed by Laurence 6 were under 20, 43 were from 20 to 60, and 14 were over 60. Of 9 cases, observed by Boudet 5 were children, 2 adults and 2 aged persons. Huss' 12 cases were all adults. Of the author's 8 cases, 1 was a child, 6 were adults, and one an aged person.

The treatment of pneumonic gangrene, whether expectant, medicinal or operative, must be governed entirely by the nature and surroundings of the individual case. These cases vary so much and essentially in their local and general characteristics that they can never be successfully managed by universal and inflexible rules. For therapeutical purposes we may arrange our cases in classes as follows: *a.* Cases in which the gangrenous focus can not be definitely located. *b.* Cases in which the gangrenous patch presumably occupies the walls of a preëxisting cavity which can be located. *c.* Cases in which the gangrenous mass occupies virgin soil, is of considerable extent, has broken down into a mass of débris and either remains unopened or has evacuated its contents into the bronchi, pleura or other space. *d.* And finally those cases in which the necrosis is of great extent—e.g., occupying the whole or greater part of a lobe, but has not yet resulted in disintegration of the tissues.

The first class of cases are open only to medicinal treatment, as are also the vast majority of the cases in the second class, although operative interference may be allowable under exceptional circumstances. In cases of the third

class medicinal measures may be all that is necessary in those in which the evacuation is into the bronchi, provided the débris is of a consistence to allow of its passage through the opening, but in other cases an operation will be generally necessary to effect a cure. In the last group of cases, heretofore uniformly fatal, we may well consider whether it were not possible to give the patient a chance for his life by enlarging to some extent the operative field.

Dr. Wells reported 8 cases and dwelt at great length upon the literature of the subject.

Dr. J. M. Patton, in being called upon to open the discussion, said: Mr. President, unfortunately I did not hear the main part of the paper, so I hardly feel able to discuss it.

It seems to me that there are two things of especial interest in regard to pulmonic gangrene, one is the question of diagnosis, and the other is *when to operate*. I do not think that we have had sufficiently large experience in operations upon lung tissue for pulmonic gangrene to make an accurate estimate of our ability to relieve such a condition by this method. One of the obstacles in our way is difficulty of diagnosis. In circumscribed gangrene it is difficult to determine whether the gangrene is from a gangrenous phthisical abscess or bronchiectatic cavity which is furnishing the secretion; unless the practitioner has had the opportunity to follow the case and determine for himself positively the relative sequence with which the symptoms will obtain in these cases, it is hard to tell which is the first to appear. If the symptoms of the cavity follow the fetid sputa it is fair to presume that you are dealing with a gangrenous portion of the lung which is breaking down. On the other hand, if the symptoms of a cavity precede fœtid sputa, it is fair to presume that you are dealing with a bronchiectatic cavity or some other cavity which is supplying the fœtid secretion. It is not always possible to tell these things positively. They are very confusing, and that fact militates more against the possibility of operating early enough than anything else.

I do not think that in diffuse pulmonic gangrene resulting from infection of the lung, pyæmia, or from the effects of infectious emboli, large infarcts, or that which occurs with pneumonia, that the possibility of gaining anything by operative interference presents very often. As a rule these cases are not benefited by an operation. They are usually fatal under all methods of treatment.

The case that Dr. Wells has cited was an exceptional one. I think a man with a large experience may run across such a case in a lifetime where the gangrenous portion is sufficiently near the surface and circumscribed to produce the symptoms he has mentioned. Where it does present the thing to do is to operate. When rupture has taken place into the pleural cavity, of course the operation should be done immediately in order to afford drainage. We need more experience in this line, and we need more operations to prove just what we can do with these cases.

The medicinal treatment of pulmonary gangrene is entirely unsatisfactory. It should be stimulating. Opium may be given for pain, but it must be carefully administered in order not to produce depression. Bromine, chlorine, thymol, menthol, creosote, and such remedies may be given as sprays or internal disinfectants, but I do not think much satisfaction will be derived from their use. Acetate of lead and tannin are recommended, but their efficacy is doubtful.

In nearly all instances it will be possible to make a positive diagnosis only by the presence of decomposed pulmonary tissue in the expectoration. When a gangrenous focus is not in communication with a patent bronchial tube there may be entire absence of fœtor or expectoration, and under these circumstances a diagnosis would be difficult or impossible.

In abscess the sputa may be purulent in character, and fœtor may not be present until some time after expectoration. In bronchiectasis the sputa may be purulent and nummular in character if the dilatation is large, surrounded by fibrous tissue, and near the surface a cracked pot sound may be obtained, the breathing is harsher, cavernous or amphoric breathing, and prolongation of the respiration is more apt to befound than in broken down lung from gangrene.

Harold N. Moyer, M.D., Adjunct Professor of Medicine in Rush Medical College, Chicago, read a paper entitled

"DUTY OF THE PROFESSION IN CASES OF INEBRIETY."

Is inebriety a vice or a disease?

To the philosophical mind it would seem as if the above question were useless, as our answer will depend almost wholly on the definition that we attach to the terms "vice" and "disease." The term disease as used by medical writers has come to have a narrow and restricted meaning that was not true of the old French word "*desaise*," from which the word is derived, and which simply meant a want of ease, or discomfort, and might be applied to an immense variety of conditions and objects. This broader use of the word was common with the early English writers; Spenser says, "Labored long in that deep ford with long disease," but of late the term has become more and more restricted until it is now largely used by medical writers to designate certain morbid conditions of the body or its organs, usually accompanied by a disturbance of function or appreciable alterations in the tissues. Later writers, particularly those indoctrinated with the evolutionary views of Darwin, Spencer, and Huxley would again enlarge the scope of this term, and now define disease as a "want of harmony between the individual and its environment." In this broad sense all vice and criminality are but diseases, which is substantially the view adopted by Lombroso, Maudsley and others interested in criminal anthropology. In this, as in many discussions, an exact appreciation of the scope and agreement as to the meaning of the terms employed would at once settle the main points of contention.

It is apparent that in the narrow restricted sense, inebriety cannot be considered a disease, certainly not in the sense that pneumonia is. Of course, the secondary effects of alcohol, the changes of brain, stomach and liver that are produced by it, are diseases; the question, however, does not relate to these but to the drink habit *per se*. At first it would seem that inebriety must surely be a vice, that to drink alcohol or to refrain from doing so is something peculiarly within the domain of the volition of the individual, and if we adopt the theological conception of free-will, we must place inebriety among the vices, but it is at this point that evolution steps forward and denies the freedom of the will in its broad sense. We are, therefore, on the horns of a dilemma, we must accept one theory or the other, which shall it be? Before answering this question, let us consider the question of utility, for theories must, to a large extent, govern practice, not only in medicine but in all human affairs. The world once burned its witches, because on the theory of witchcraft they believed it would be dangerous to allow witches to live and be at large. The revolving stool, baths of surprise, and chains were part of the necessary outfit for the treatment of insanity. It was not that the people of those times wished to be cruel, but the treatment was the outgrowth of a mistaken theory of insanity. This theory was based on the metaphysical conception of free-will. The doctors interrogated their own minds and found that they had the power to control their wayward fancies and coördinate their conduct. They believed that lunatics had the same power if they were only furnished a sufficiently powerful motive, hence the stripes and chains with which lunatics were treated and, according to their theory of diseases, the treatment was certainly appropriate. They could not conceive of the mind being diseased, hence insanity marked but an imperfection in the will. The same question is now raised concerning inebriety, and we must ask ourselves whether the chronic drinker *will not* or *cannot* abstain from liquor. If the liquor habit marks but an imperfection of the will, its natural treatment will be by prayers, exorcisms, and moral suasion as well as fines and imprisonment. The world has been following this prescription for many years, and with so far but imperfect results. The agitation against liquor has greatly lessened its use as a beverage, and has quite banished it from the tables of the better classes of the com-

munity, but at the same time we doubt if there has been a material decrease in inebriety. Certainly not if we count the various drugs habits that, in a measure, have taken the place of alcohol. The chief argument that is used in support of the vicious theory of drunkenness is that to admit for a moment that the inebriate is a diseased person, removes at once all incentive to moral regeneration, that he will sink supinely into the condition of an interesting invalid, and will give up all hope of reformation. We do not think that this view is well taken or that it is borne out by the experience of those skilled in the treatment of inebriety.

If we tell a gouty person that his disease is the result of an indiscretion in diet, and that he must be more abstemious in the future, do we thereby lessen his inhibitory power? Would it be better to say that there is nothing the matter with him and that the pain in his toe marks but an imperfection in his morals?

If we assume that inebriety is a disease, we place an additional restraint upon the use of alcoholic liquors. If the consequences of over-indulgence are once clearly pointed out, and that the chronic ingestion of alcohol not only produces a disease, but in a measure renders an individual irresponsible, not only will there be an additional incentive to refrain from its use, but the community will deal much more justly and vigorously with the inebriate. The chief enemy of these unfortunates is their intense egotism. It is rare to meet one who does not say that he can give up the habit any time, or after a brief period of abstinence will say that he could wade through a lake of liquor as deep as his chin and not drink a drop. Such remarks as these are usually precedent to a most disastrous relapse. The only way to overcome this egotism and the too great confidence of friends, is to teach that inebriety is to a great extent beyond control of the person's will. That the mind and body are diseased, and that it is necessary for the persons to place themselves under medical advice for the bodily ailments and under the best circumstances for aiding the feeble will.

Another matter that has served to obscure the discussion and proper understanding of this subject is that there is no well marked line of division between liquor taking and inebriety. One swallow does not make a summer, and one drink does not make an inebriate. In fact, some individuals may and do take liquor in moderate quantities for many years, without becoming inebriates. It is impossible to lay down any hard line on one side of which a man shall be said to be an inebriate and on the other that he has the drink habit. It would be desirable for our theories if Nature would follow our classifications and make clean cut distinctions. We can divide the whole world into inebriates and sober people, but we shall find all gradations, from the individual who takes three or four drinks a year, to the victim of the drink disease who is as hopeless and helpless in the presence of his malady as is the sufferer from epilepsy or hydrophobia. This want of division is not peculiar to inebriety, it is characteristic of insanity, and one of the chief difficulties in dealing with the legal control of the lunatic arises from the want of a dividing line between sanity and insanity, a difficulty that our judges have been attempting to overcome for over two hundred years. It is impossible to lay down any definite rule, but in a general way, where there is a distinct craving for alcohol, which is only partially restrained, or where we have unrestrained indulgence with the more constant phenomena of acute or chronic intoxication, or where we have the secondary effects of alcohol upon the tissues, we may make a diagnosis of inebriety. It is well understood that such division is but arbitrary, and the effects of alcohol in the milder cases is one of degree and not of kind. Still, a division must be made somewhere, and it would seem best to place it near the point where alcohol affects self control and civic relations.

The profession at large owe a great debt to the inebriate, and one that they appreciated too lightly in the past. It is rare that a patient presents himself directly for the alcohol habit; it is usually for some intercurrent affection or for the secondary results of the alcohol. As a rule, the symptoms are prescribed for, a few inquiries are made regarding habits and the patient is told to moderate, or stop his use of alcohol, and dismissed with a few added injunctions as to diet. In these cases the physician regards as a *cause* that which is in reality the disease (inebriety). It is the duty of the physician in every such case to make a careful inquiry into the family and previous history of the patient, the date of beginning of the alcohol habit, its cause if ascertainable, and finally the quantity, time and kind of alcoholic drinks taken. These, with an estimate of the condition of the nervous system and an examination of the internal organs, will furnish a guide for prognosis and the general indications for treatment.

I do not believe there is any specific treatment for inebriety nor that there is any drug or combination of drugs that will relieve the craving for liquor, except they are themselves intoxicants, or are like strychnia, atropia or other alkaloids, that when exhibited in very large doses produce a marked toxic effect. Not that these drugs are devoid of value when exhibited in proper doses, and under proper directions. They are most excellent tonics and restoratives, but when given in extraordinary quantities and particularly beneath the skin they may produce very profound disturbances in the nutrition of the nerve centers that may lead to insanity or the production of the more grave degenerations. Such drugs as Ext. Cocoa and the general nerve tonics are indicated but they are not to be given in doses that shall in any way make them substitutes for the alcohol that has been taken. Hot baths are useful, especially the Turkish bath where available; they tranquilize the nervous system and aid elimination. Chloral, the bromides and other sedatives are useful to procure sleep. These patients ought to be kept busy as much as possible with their treatment. Medicines should be given often, and in small doses and the baths frequently repeated. It is astonishing sometimes what a wonderful effect, a small bottle of a comparatively innocent drug will have in allaying the thirst for liquor, providing the patient is told with sufficient earnestness that it is given for that purpose. In a general way, the home treatment of inebriety, like that of insanity, is unsatisfactory. In the milder cases, good results may be achieved. In the more advanced cases, where the peculiar mental changes of chronic alcoholism have come on, it is better to send the patient to some institution. In this way, old habits and associations are interrupted and a more profound impression is made upon the mind of the patient than can ever be reached by the physician at home, be he ever so skilful.

In bringing this paper to a close, I am profoundly impressed with its fragmentary nature. I have made no attempt to go into the extensive and valuable literature with which, indeed, I am but imperfectly acquainted. I have simply endeavored to set down a few suggestions that have been the outgrowth of my experience with this class of cases. If it shall but stimulate the profession to a livelier sense of their responsibility in dealing with these unfortunates, it will fulfil the object for which it was written.

Dr. J. J. M. Angear: I have been very much interested in the paper just read. The great difficulty that I have realized in obtaining reliable facts from the literature upon this subject is that it comes from two very extreme sources, the liquor loving man, that is, the man who is the habit of drinking, and writes for the purpose of backing up his own habit. The other extreme is the man who does not think that alcohol is ever to be given as a medicine, and that all the crimes that were ever committed from Adam down to

the present time have been done directly by the influence of alcohol. While we have such terrible extremes we are never going to arrive at the truth.

With reference to the disease as it was brought out in the paper, I think that we should all have that clearly defined in our minds. I have so far arrived at this conclusion, that *it is a habit* primarily. Habit begets a taste and the taste may be hereditary, and when a patient has gone far enough into this habit there must be a diseased condition of the brain as well as of the stomach, the lungs and the kidneys. The disease of the brain is manifest by psychical symptoms, hence it would come in the domain of insanity. As it has been stated in the paper that it affects the will and moral faculties, I believe that the time comes when the individual is entirely beyond redemption; I think that he is so far over the cataract that there is no power on earth that can save him, and whether there is any power in Heaven to save him is another question. It is too late to talk to him about the evil that he is doing both to himself and family. I think we may as well talk to a pair of old boots as to talk to an individual when he has reached this point, because he has lost his manhood, and he is lost, I was going to say, to all eternity. It now becomes a duty of somebody to put him where he cannot get alcohol in any form. Would it not be better to do this before he reaches this point, and thereby save him?

I have had an opportunity of watching these cases, particularly in Iowa, during the Murphy and Bonticue movement. The idea was brought out in the paper and I wish to endorse it, that these men were given to egotism. I have seen men get up in these meetings and brag—for I can call it nothing else—of how many times they had been down in the gutter, but now they had drunk their last drop of the infernal stuff. These men would talk loud and with great flourish and the probabilities are that shortly after these meetings they were in the same gutter that they depicted. They will get up in meetings, confess their sins, and then they are forgiven, so they say. They keep on doing this perhaps forty times a year. I have seen it so often that I fairly tremble when I hear men speak of how low down they have been, what dogs they have been, etc. I know such men are on the verge and they have not stability to prevent themselves from falling headlong over the precipice into a drunkard's grave.

Dr. F. C. Schaefer: In reference to the point of heredity, there is a great deal to be said. I remember one instance of a young man, who, after having tasted liquor, could not stop drinking until he found himself in the gutter. I knew his father quite well. He was a middle aged man, and was not a drunkard in early life. He had three boys born before he took to the drinking habit. They have been and are still sober men. After having been an inebriate for the greater portion of ten years, his wife presented him with another son, and this was the boy who became a drunkard. He was remarkably exemplary in character in every respect until he began to drink at the age of 25 years, and finally died a drunkard at the age of 41. Doubtless many similar instances might be related, but this case struck me very forcibly as having a practical bearing upon the question of heredity.

Concerning loss of will it sometimes seems doubtful if these men do lose their will power. They exert a wonderful will power over others in obtaining whisky, but there are people who may lose it entirely. I would not say it is not so. A sad case occurred in this city several years ago. The gentleman was a physician, and doubtless some of the members here were acquainted with him. He died at the age of 55, having been a drunkard for 20 years. He was said to have diabetes during his last illness. I called to see him a number of times in a social way. He had symptoms of gastritis, and I told him one day that he had no diabetes but that the glandular structure of his stomach had been ruined by the use of whisky. He said, "That is true, doctor." For about six months he did not take as much nourishment as a cupful of milk per diem in addition to one-half pint whisky. Finally, when his end was drawing near a neurologist was called in, who suggested that we had better cut off his supply of whisky. It seemed useless to do it at this time as the man was on the verge of the grave and could not live more than two or three weeks. "Well," said the consultant, "I think we had better cut it off," and I told him what the doctor had said. He then replied "Dr. Schaefer, if the devil was at the door beckoning me I would not stop drinking whisky," and he continued drinking until relieved by death twenty days later.

Dr. John D. Skeer: I have been under the impression that the constant use of alcohol changes the condition of the brain from a normal to an abnormal condition, which subsequently becomes the normal condition of the brain from the constant desire to use it, and the individual craves for alcohol just as much as a man craves for butter and milk or any other article of food. It seems to me that explains to my mind in a very satisfactory way the irresistible desire that men have for the use of alcohol.

Dr. Edward F. Wells: I would like to ask the reader of the valuable paper to which we have listened whether he considers drunkenness a common cause of insanity, as I understood that to be the tenor of a portion of the paper?

Dr. Moyer: Not inebriety. The use of alcohol is an important factor in causing all varieties of insanity.

Dr. Wells: In this connection I wish to call attention to one fallacy in statistics relating to the cause of insanity. The etiological statistics of insane hospitals are usually made up from the original papers giving the alleged cause of the disease, and that alleged cause is that which is described by the next friend of the patient in his petition that the party may be declared insane. Such persons are usually not medical men, and the most obvious feature of the patient's habit is seized upon at once as the cause of his disease. If he hesitates as to the cause, the accommodating magistrate is very apt to say, "What is the cause of this? Does he drink?" "Yes, he was in the habit of drinking." Whereupon the case is put down as being due to drunkenness. If this is not the case, he will say, "Has the man met with reverses, or a mental shock of any kind?" "Well, yes," and then that is put down as the cause. "Has he met with an injury of the head?" "Yes. When a child he fell out of a dray and hurt his head," and this cause is assigned. Such cases are utterly worthless in a scientific point of view for the purpose of investigating the causation of diseases. Before it can be asserted that drunkenness is a cause of insanity, it must be proven that the disease exists in persons addicted to this habit in a distinctly larger proportion than amongst others, and this has not yet been done. Were drunkenness a factor of great importance in the causation of insanity, we would expect to find in countries where drinking is carried to extremes a much greater percentage of the disease than we find in those countries where it is not carried to an extreme. Statistics do not bear out this point. To be sure a great number of insane patients have been drunkards, and a great number of drunkards exist who are not insane.

Nine persons having found their way into one of the insane hospitals within a comparatively short period, following closely upon the so-called "bichloride of gold" treatment for drunkenness, we would naturally conclude that the treatment was the cause of insanity, because the proportion to the whole number treated is, comparatively, very large.

In regard to the management of drunkards. The treatment pursued by Dr. Keeley, at Dwight, I conceive to have one element of success in it, and that is this: Patients are brought together in large numbers, seven or eight hundred, and great enthusiasm is found to exist, and this enthusiasm is most assiduously fostered. A patient going there must necessarily be a man of the better class, inasmuch as the charges are such that people in the lower walks of life cannot get there. Such a man takes a stand in favor of reformation and temperance and it puts him on his guard, and his position is placed in jeopardy, so that when he returns, as he says "cured," his pride may hold him up longer than would the ordinary treatment at home. If that person were now surrounded by proper elevating influences and not thrown amongst old companions who were in the habit of inviting him to take a drink, he might have an opportunity of becoming permanently cured. In addition, were he given remunerative, responsible and laborious work his prospects would be greatly augmented. As a rule, however, I think Dr. Moyer's four cases of relapse are fair samples of the majority of cases that "graduate" at such institutions.

Dr. J. M. Patton: I have been interested in this subject of late and want to learn something from the general discussion, but I confess being much mixed on the subject. I have been unable to discover that alienists, who ought to know as to whether inebriety is a disease or not, whether it is hereditary, and the probability of its cure, know very much about it. The fact is they are puzzled as much as any other class of men. Take, for instance, the definition which Dr. Moyer quoted. He said it consisted in want of harmony of individuals with their environment. It did not seem to me that that covered the point. These men's surroundings are saloons and if they are not in harmony with them, I would like to know what they are in harmony with.

In regard to its being a disease, in the first place we have no definition that is reliable, and I do not think it is necessary for us to have any pathological condition. We recognize diseases of the brain in which no pathological changes can be demonstrated, and therefore I do not think secondary organic changes in the body which follow from alcohol have any more connection with the primary condition than if those changes were in the liver or stomach, which take place as a secondary result of disease of the heart for instance.

Then again in regard to its curability, we have all manner of claims for that. I do not believe any more than Dr. Moyer does that we have a specific in any one drug or any one class of drugs for its treatment. I think the question is a moral one after all; and yet in looking over the literature for the past two or three years, which has been quite extensive, as a general practioner who is seeking after some knowledge, I confess I have been unable to find much. I am just as undecided what to do and what to recommend these patients to do as I was before. I feel like condemning the want of energy shown by the alienists in discussing the effect of the various cures for inebriety on the public at large. Very few of them will recognize a scientific basis for the cures. I think it is on them that both the public and physicians do much depend for confirmation in this matter and the efficacy of the Keeley or any other cure. It is due to the profession and the public, that, if they do not believe in the methods adopted at various sanitories, they should openly condemn them.

Dr. Harold N. Moyer in closing said: It seems to me that considerable has been laid out in the discussion. Dr. Angear says that alcohol has been charged with all crimes since the fall of Adam. The Garden of Eden affair was not a case of inebriety, but one of serpent poisoning. He takes the ground that it is a habit, but does not define what he means by habit. A person may have a habit of getting up early in the morning; he may have a habit of staying out late at night. I would like him to tell us what he means by the word habit.

Dr. Angear: I should have done so if the President had not shut me off.

Dr. Moyer: I fear Dr. Graham has misapprehended some points in my paper. The original word disease was a broad term that later narrowed by modern medical writers, in the hands of such writers as Huxley, Darwin, Spencer and others the term again broadened, and the definition as quoted by Dr. Patton that disease was "a want of harmony between the individual and his environment," is generally accepted by the evolutionary school. I clearly stated in one sentence in my paper that inebriety was not a disease in the sense that pneumonia is, but this is true of some forms of insanity, notably paranoia. Inebriety is not a disease in the restricted use of the word. You remember that I said that one drink did not make an inebriate; but that where drunkenness affected the civic relations, or where one is helpless to resist the craving for alcohol or the peculiar mental changes of chronic alcoholism have come on, we should make a diagnosis of inebriety. That is not a hard and fast line. Many have delusions who are not insane. Many have mental peculiarities which only need to be exaggerated to become insanity, yet the law attempts to make a division somewhere, I attempted to lay down some sort of rule by which we could measure the use of alcohol and state where the drink habit passed into inebriety.

I have not seen the paper of Dr. Dana, which was referred to by Dr. Graham, but I know that anything that Dr. Dana writes would be valuable from a scientific standpoint, and I think I can agree with Dr. Dana as to the proportion of inebriates—one to seven hundred. I have thought a good deal over this matter and my own idea would be about one in every three or four hundred. There is a large number of men who drink who are not inebriates. Men die of hobnail liver who are in no sense inebriates. I agree with Dr. Wells that the etiological statistics of insanity are absolutely valueless as they appear in asylum reports. The cases mentioned were carefully studied and the sources of fallacy pointed out by him were excluded.

Dr. Frederick C. Schaefer reported a case, and exhibited specimen, of

ABSCESS OF THE LIVER FOLLOWING TYPHOID FEVER.

The case I have to report to-night is that of a colored man, aged 39 years, who was taken with typhoid fever on or about the 5th of last August. He desired me to treat him. Expecting to leave the city for my summer vacation in about three weeks from that time, I at first refused. He said he would like to have me take care of him until my vacation time, and then send another doctor. With this understanding I treated him for three weeks, the temperature running up to 103° F. at night, and 101° F. in the morning; the pulse ranged between 100 and 120. He had an average temperature of 101° during this time. About the 1st of September I went on my vacation, and Dr. Haven kindly took charge of the case. I saw the patient again some thirty days thereafter. He had at this time a higher temperature than ever, 105° F. Dr. Haven stated that the fever had run a mild course during the first three weeks of my absence, and towards the last week in September the man had chills, the temperature rose to 104° F., and he complained of more or less pain about the region of the liver. Dr. Haven after this was unable to see the patient, and Dr. Skeer visited him a few times. There was slight improvement for a few days, but at the end of another week he had chills again, with elevation of temperature and fever continued. One day the temperature ranged 102-3°, and then it came down to 100° F. For two or three days the temperature was normal, then it rose a degree or two again. About the middle of October, I concluded that an abscess of the liver had formed. There was great tenderness over the region of the liver, he had slight cough and marked dyspnœa, and from the latter part of September until this time the organ grew larger. Patient was nauseated and bowels constipated. I told his wife that her husband had an abscess of the liver; that an exploratory incision ought to be made, and had her speak to him about it. He objected. Feeling that they did not realize the gravity of the situation, I urged that another doctor be called in to see him with me, and if he agreed it was abscess of the liver I would open it with their permission. They called in Dr. Newman, who was their physician years ago. After making an examination, he came to the same conclusion, and said the proper thing to do was to make an exploratory incision to see if we could not drain it. The man refused to have it done at this time. For three or four days he refused, and said he preferred to die a "natural" death. During the latter part of October I felt it was wrong to treat him longer without opening the abscess; we called Dr. Skeer in consultation. He examined the case, and agreed with me that the patient had an abscess of the liver. We found dulness on percussion as high as the fourth rib. He still refused to be operated upon. During the first week of November, I told them I must operate or withdraw from the case. Dr. Curtis then treated him right along, and after another month or six weeks the patient developed ascites. The doctor tapped him and drew away a quart of ascitic fluid. On the 19th of last March they called me in consultation.

The doctor said the abscess pointed below the last rib at the side a week before this time, and with a mere scratch of the scalpel he perforated it. A gallon of pus was evacuated. On the 21st of March I made an autopsy, in the presence of Drs. Skeer and Curtis. We found an abscess on the upper surface of the right lobe of the liver. There was a cavity there that would hold two fists. We found that the liver was adherent to the parietal peritoneum some 3 inches below the last rib, where the abscess pressed upon the hepatic flexure of the colon. The liver had rotated upon its transverse axis forwards, so that the left lobe was thrown up under the diaphragm. It was much contracted, having evidently been forced up there, and the adhesions of the abscess wall to the parietes had held the right side of the liver down so that it rotated forwards, while the left lobe extended up under the diaphragm. The condition was somewhat different when the organ was distended with pus. The capsule of the liver was stretched. When Dr. Newman was with me I succeeded in passing an aspirator needle into the liver, but

obtained nothing. He would not permit a second needling. I neglected to say that there was a slight jaundice. Our patient had always been a sober, industrious man, who never drank alcoholic beverages of any kind, and had never been sick before.

Suppurative Hepatitis.—Among the causes which give rise to liver abscess mentioned in different text-books are:

1. Traumatism.

2. Extension of suppurative, inflammatory and ulcerative processes from contiguous structures.

3. Emboli in the blood-vessels of the liver.

4. Echinococci.

To these, in the light of recent developments in pathology, I may safely add:

5. The arrest of pyogenic germs or their ptomaines, in the liver structure, conveyed there by the blood-vessels or the lymphatics.

It is true that the germs frequently constitute a part of an embolism, and many times are conveyed in this manner to the liver from some suppurative thrombus.

In this connection it will not be necessary to refer to traumatic hepatitis. The second cause may be simply alluded to. Broussais long ago claimed that gastro-enteritis was a common cause of the disease, the inflammatory process extending from the duodenum into the biliary ducts, from there into the liver. Although this was at one time regarded as the *fons et origo* of most abscesses of the liver, the ideas of the profession have undergone a change as to this theory; still admitting that abscess of the liver does occasionally develop in this manner. Cases have been reported of suppurative hepatitis having extended to the liver from suppurative peritonitis, perinephritis, also emphysema, inflammation of the vertebræ, colon, and perityphlitis.

A more frequent origin of hepatic abscess is traceable to suppurative inflammation of the mucous membrane of the gall bladder and gall ducts, and the presence of stones in the bladder or duct. The latter causing suppurative inflammation of the tissues in which they happen to be located, this inflammation is frequently followed by ulceration, with perforation and extension of the inflammatory process into the liver, by reason of contact with the latter viscus. The former process, suppurative inflammation of the lining of the gall bladder and duct (caused usually by the presence of stone), may lead to suppurative inflammation of the liver by developing thrombo-phlebitis, or by transmitting the pyogenic germs directly to the portal vein, for the veins of the gall bladder and ducts pour their contents into the portal vein. Again, a stone impacted in the ductus choledochus may, by pressure upon the portal vein, lead to the formation of thrombus in the latter. Geigel reported a case of this kind in 1890 (*Sajous' Annual*, 1890).

Probably the most common of all causes of suppurative hepatitis are emboli in the vessels of the liver: *a*, portal vein; *b*, hepatic artery; *c*, hepatic vein—the order of frequency being in the order mentioned.

Abscess resulting from phlebitis in the radicles of the portal vein, the hæmorrhoidal, mesenteric, pancreatic, duodenal, splenic, pyloric and gastric, including also the veins of gall bladder and ducts—this is self-evident. Diseases of the rectum have been a fruitful source of liver abscesses, *via* the hæmorrhoidal veins. It is a well established fact that in young children phlebitis of the umbilical cord has been followed by suppurative hepatitis. Ziemssen mentions a case of abscess of the liver whose source was traced through the pancreatic vein to an abscess in the pancreas. Burk found an infarct in the spleen which he conjectured led to the formation of suppurative hepatitis, multiple abscesses having been discovered by him in the liver. Ackerman found thrombi in the vena gastro-epiploica dextra, accompanied by hepatic suppuration. Libermeister (Ziemssen's Cyclopædia, Vol. ii,) mentions abscesses of the liver as a sequela of abdominal typhus. Virchow discovered (*Arch.*, Bd. 1) infarcts in the lungs from which he claimed putrid clots were formed in the pulmonary vein, fragments of which plugged the superior mesenteric artery, and metastatic gangrenous deposits occurred in the liver, brain and kidneys. Meyer (*Berl. Klin. Wochenschrift*, 1868, No. 42) found a similar condition from ulceration of the endocardium.

Emboli have been conveyed through the hepatic artery. (O. Weber, *Deutsche Klinic*, 1867.) By way of hepatic veins it is claimed emboli may enter the liver. Cohn made experiments to prove this by injecting fine wheat meal covered with Canada balsam into the jugular of a rabbit, then compressed the thorax, and at autopsy found particles of the meal in the finer twigs of the hepatic vein. General pyæmia from any cause, and thrombo-phlebitis from any point from the head to the great toe, may have among their sequelæ hepatic abscess.

Symptoms are not always well marked, and as they are given in our standard text-books, it would seem unnecessary to occupy your time in a rehearsal of them. I would, however, call your attention to three differential symptoms, or rather signs, mentioned by Cyr (*Sajous' Annual* of 1888), which he claims will sometimes expedite the surgeon's effort in locating the abscess. They are:

1. When the abscess is located in the front convex surface, there is dyspnœa in addition to marked pain radiating towards the chest and shoulders, jaundice being exceptionally present. (This sign was present in a marked degree in the case here reported.)

2. When the suppuration is limited to the central part of the organ, local symptoms are more or less absent, either of the liver itself or of the neighboring organs, and decided jaundice exists if the abscess is large.

3. If the suppurative inflammation is limited to the under concave surface, there is entire absence of the thoracic symptoms with, at the same time, stomachic troubles, especially uncontrollable vomiting; the pain radiating in a downward direction.

Dr. Cyr gave statistics of 563 cases. Fifty-five per cent. died without the abscess being opened; 14.9 per cent. were opened with the knife; 10.5 per cent. opened spontaneously into the lung; 7 per cent. opened into the peritoneum; 5.5 per cent. opened into the pleura; 1.9 per cent. opened into the colon; 7 per cent. opened into the bile duct; 3 per cent. opened into the vena cava; 1 per cent. opened into the peritoneum.

Dr. Ferron gave statistics of liver abscess treated by hepatotomy (*Annual*, 1888), 49, 10 deaths, of which 5 had multiple abscess. One was in bad general condition, having had a collapsed lung; 3 died of independent causes; 1 died of peritonitis on fifteenth day; 37 recovered, 70.6 per cent. —showing a difference of about 26 per cent. in favor of operation.

With this remarkable record before the profession, it would seem to be our positive duty to operate whenever possible, provided always that the abscess has been discovered before necessarily fatal complications have arisen, and while our patients have a reasonable degree of strength left to withstand the shock. These are the two operations recommended: Needle—aspirate; knife—hepatotomy.

Is it wise to use the needle through the parietal wall? I think in a small percentage of cases only, and those which form a prominent projection towards abdomen or below the last rib at side or back. If we have multiple abscesses, we may be led astray by successfully reaching one and failing to find the others. I would in most cases prefer exploratory laparotomy; then use the needle to locate the focal point if

possible. With a hand in direct contact with the viscus, we are in a position to find the abscesses with far more accuracy and expedition. Having located the focal point, the most reliable procedure is hepatotomy, either in one or two operations. Tait's method with single incision recommends itself, as we thus have to etherize and terrorize our patient a less number of times. Should, however, plausible reasons arise for the extra precautions, it may be wiser to resort to the double operation, which consists in first cutting down to the liver and stitching it to the abdominal wall, and giving three to four days for adhesions to occur. Should we decide upon the single operation, we can do so as follows, according to Tait:

"*Operation.*—The parts being cleansed and the other preliminary steps taken, make an incision about 4 inches long over the most prominent part of the swelling, down to the peritoneum. All hæmorrhage is next arrested, and this layer carefully slit up. The liver is now recognized, and carbolized sponges or towels are carefully packed in on either side, so as to prevent any escape of fluid into the peritoneal cavity.

"The needle of an aspirator, or a fine trocar, is then thrust in, and the existence of fluid beneath thus verified. As the needle is withdrawn the liver is incised, and a finger quickly plugs, and then enlarges to 1½ inch, the opening made by the knife. Hæmorrhage, if free, is easily arrested thus, or by sponge pressure. Escape of fluids into the peritoneal cavity is prevented by the use of the sponges already mentioned, by an assistant keeping the edges of the wound carefully adjusted to the liver, and lastly, by the next step, which consists in hooking up the opening in the liver with the finger or forceps, and in stitching the edges of the wound in the liver to that in the abdomen with a continuous suture of carbolized silk. In inserting this, care must be taken to unite peritoneum to peritoneum, and to take up a sufficiency of tissue by inserting the needle well away from the edges of the wound. As the suture is inserted the sponges must be gradually withdrawn, and if the fluid escapes very freely it may be well to turn the patient over on his side. All handling must be of the gentlest. A large drainage tube is then inserted, and dry sal alembroth or iodoform gauze dressings applied."

Dr. J. B. Herrick: Dr. Schaefer has given us an excellent *resumé* of the subject of abscess of the liver and its etiology. As he has said, an abscess may be caused by traumatism, by extension of inflammation from surrounding organs, or by infection through the avenue of the bile ducts or of the circulation. Under the head of infection through the avenue of the bile-ducts, I do not think, however, that it is always necessary to assume a phlebitis in the radicals of the portal vein that run in the walls of the gall-bladder or bile-ducts. When, for instance, a gall-stone is impacted in the bile-duct, it causes sufficient irritation so that microörganisms traveling up the bile-duct find here a point of minor resistance, and a localized suppuration in the bile-duct is the result. And by direct extension this localized suppurative cholangitis may become diffuse and involve the bile capillaries. We may thus have by penetration of the wall of the bile capillary a suppurative inflammation in the parenchyma of the liver, and this without any phlebitis in the portal radicals.

Late investigations have shown that there are many abscesses of the liver that are accompanied by the *amœba coli*. Experiments and researches by Loesch, Hlava, Koch, and especially by Kartulis, of Alexandria, who reports some five or six hundred cases, and in this country by Osler, Councilmann and Lafleur of Baltimore, have shown that dysentery in one of its forms, is doubtless caused by an amœboid body to which the name amœba coli or amœba dysenteriæ has been given; and in abscesses of the liver that arise as a consequence of dysentery, the amœba has been found as well as pus producing microörganisms.

Kartulis is of the opinion that the amœba coli is not a direct cause of suppuration in the liver, but that it is the carrier of the pus microbe. His idea is that the amœba coli carrying with it pyogenic bacteria enter the portal vein, possibly by virtue of its amœboid movements, is carried in the vein to the liver, and excites there suppuration. Or a thombophlebitis occurs in the radicals of the portal vein, and a detached embolus containing the amœba and pus microbe is lodged in the liver. Councilmann advances the theory that an abscess of the liver may be formed by the amœba coli passing through the wall of the intestine, lodging in between the diaphragm and upper surface of the liver, there penetrating again the serous coat of the liver and exciting a suppurative inflammation in this organ. This theory he suggests to explain the frequent occurrence of abscess of the upper portion of the right lobe of the lung, the two occurring together in several of his cases as well as in numerous cases reported by other observers. The question as to the amœba coli and its relation to abscess of the liver, as well as to dysentery is an extremely interesting one and one needing still further investigation.

There was one point, as it appeared to me, not clearly enough brought out by Dr. Schaefer, and that is with reference to the diagnosis of typhoid fever in the case in question. From the report Dr. Schaefer makes, we have no proof post-mortem that the case was one of typhoid fever, and I am sure from the experience I have had in the County Hospital that an incorrect diagnosis in an abscess of the liver can very readily be made.

I well remember two cases that went from the wards to the morgue that had been treated for typhoid fever. In both we found abscess of the liver and not the slightest evidence in the intestines or glands of typhoid fever. In one case we could not account for the origin. In another there was abscess of the liver from gall-stones and suppuration in the bile-ducts and gall-bladder. It seems we have a right to ask for more positively conclusive symptoms of typhoid or post mortem evidences that typhoid was the original disease, and that there did not exist some other cause than this for the abscess of the liver.

Dr. H. N. Moyer: In the symptoms mentioned Dr. Schaefer neglected one which is of some importance, that is, the presence in the urine in these cases of peptones or some other substance which gives a similar reaction. Of course, in a general way, it is not by any means indicative of hepatic abscess. It shows that there is pus incarcerated in an organ, and where the products of bacterial infection are going on, it would be present in empyæma. But in these cases early diagnosis is a *desideratum*, it is not always easy to reach. In several of the cases I have seen, a diagnosis has been made much too late. This sign is one of some value.

Dr. Weller Van Hook: I wish to say a word or two in regard to the very interesting report of Dr. Schaefer. The principal point I had made up my mind to present was the one mentioned by Dr. Herrick in regard to diagnosis. The studies of the amœba coli in reference to its relation to abscess of the liver have dated back to the year 1875. The first case of abscess of the liver recognized to have been associated with the amœba was reported by Loesch in that year. Since the time of Loesch there have appeared extensive works by other investigators. Kartulis of Alexandria has established the idea that amœba was the cause of tropical hepatic abscess indirectly, i.e., through a primary "amœba dysentery." Massintin has claimed that the amœba is found in the colon in other diseased conditions, in ordinary acute and chronic diarrhœas and in typhoid fever as well. He goes so far as to report cases of typhoid fever in which he found amœba. Kartulis has contested his claim for having found amœba in the intestine in typhoid fever, on the ground that the diagnosis of typhoid was incorrect. So it behooves every one who publishes a case of abscess of the liver that has arisen in the course of typhoid fever to be sure of his diagnosis especially if a search has been made for the amœba coli. If we can have some statement in regard to post mortem findings in addition to those which Dr. Schaefer has given us, they may be valuable in reference to diagnosis.

The echinococci, mentioned as having been the cause of liver abscess, of course Dr. Schaefer referred to only as an indirect cause of suppuration; that is to say, by causing pressure atrophy of the surrounding parts the echinococcus cysts may become septic by opening up the intestinal or other tract infected.

As regards the action of the amœba coli in carrying the pyogenic microbes, the ideas in regard to the inclusion of these pathogenic bacteria and their transmission to the liver by the amœba, though very plausible and probably correct, do not find recent confirmation. Dr. D. Nasse (Archiv. für Klin. Chir., XLIII, 1, 1892), found in his careful study of a case of liver abscess no evidence that this was the case.

It cannot be sustained that gall stones are a very common cause of hepatic abscess. Gall stones do result in the forma-

tion of empyæma of the gall bladder. A far more common cause of liver abscess is the embolic one. According to the statements of Orth, a very large majority of the cases of non-tropical abscess of the liver are due to embolic processes.

It is interesting to note that Kartulis has succeeded in cultivating the amœba, although he has only once succeeded in obtaining a pure culture. Experiments upon cats developed a typical inflammation of the large intestine with characteristic ulcers in which the amœbæ were found on examination of microscopic sections.

Dr. J. M. Patton: I would like to ask Dr. Moyer why he considers peptonuria a special sign of the presence of pus. According to the theory advanced by Koettnitz in regard to the etiology of leucocythæmia, peptonuria in that instance occurs because there is a failure of the natural process of transforming peptones into various forms of albumin, or the globulins, which has been established by Hofmeister, and that transformation not taking place, the leucocytes, which are abundant in the mucosa of the intestinal canal, carry into the blood a large proportion of free peptones, and therefore peptonuria is simply a symptom or an evidence of leucocytosis. If that is a fact, I do not see how it should be received as an evidence of pus, because leucocytosis need not necessarily mean a tendency to the formation of pus. Is it a fact that the presence of pus in the body is shown by peptonuria with sufficient certainty to warrant our accepting it as a symptom of pus formation?

Dr. Harold N. Moyer: The presence of peptones is not constant where pus is incarcerated in the body, but it occurs with sufficient frequency to give the sign some value in such an obscure disease as hepatic abscess. Where we have some symptoms pointing indefinitely to this condition, it might exclude other things. There is some question as to whether this reaction is due to peptones. The reaction I mean is obtained with picric acid. The albumin, if any, is precipitated by heat, and the urine is overlaid with a saturated solution of picric acid. If a cloud appears at the point of contact that does not clear up by gently heating, I regard peptones as present. It makes little difference whether it is a true peptone; the main thing is the reaction.

Dr. Patton: Does that reaction obtain in cases of incarcerated pus?

Dr. Moyer: Yes, I think it does. I have obtained the reaction in a number of my own cases, but my experience has been so limited that I am largely basing my remarks on the literature of the subject.

Dr. Angear: I would like to ask Dr. Schaefer if he noted anything peculiar in the fæcal matter? I will simply state my reason for asking that question. I have been somewhat interested in the study of the function of the liver. With our idea of the amount of liver substance that must necessarily be destroyed in this case, we should suppose that there was a less amount of bile thrown into the alimentary canal, and there should be some evidence of disturbed condition of the fæces, such as, for instance, fermentation. We are taught, and have reason to believe, that the bile prevents fermentation, and that very frequently we have diarrhœas of a yeasty character, that we ought to conclude is evidence of lack of bile thrown into the alimentary canal. The profession, from time immemorial, have been looking into the chamber vessels for evidences of bile, and if they had dark fæces it is bile, they thought, when in fact the color of the fæces comes from the large intestines, and not from the liver. In looking over many works on diseases of the liver, I do not find the clay-colored or white stools, nor the frothy diarrhœa, spoken of as often as we might expect in diseases of the liver—hence my reason for asking the question whether there was anything peculiar in the fæces noted.

Dr. F. C. Schaefer, in closing the discussion, said: With reference to the point raised by Dr. Moyer, I will say it was not my intention to give all of the symptoms of liver disease as they are mentioned in our numerous text-books, and do not wish him to understand that I ignored the symptom he mentioned by any means. It has some bearing on the diagnosis.

In reply to the remarks made by Dr. Herrick concerning the diagnosis of the typhoid fever, that is one of the most important points, and should have been dwelt upon a little more fully. I remember distinctly that this man had the usual symptoms of typhoid fever, with the exception of diarrhœa. Of course, we have typhoid fever without diarrhœa. There was marked tenderness in the right iliac fossa, great tympanites in a very short time. There were petechial spots, although it was difficult to define them accurately, on account of the color of the skin. The man had headache and bled from the nose—which, however, are symptoms of liver disease as well. The course of the fever was typical of typhoid, with the usual variation in the morning and evening temperature. The marked chills which occurred six or seven weeks after the onset of the fever, were indicative of some pathological change going on in the body which might have been secondary to the typhoid; at least, one would naturally look for complications coming on at that period.

I was positive in reference to the diagnosis, and do not think Dr. Skeer doubted the diagnosis when he saw the case, nor did Dr. Haven. It was with the utmost difficulty that we could make an autopsy. The intestines were of a milky-white color, almost bloodless. In the abdomen I felt some large mesenteric glands; a few of them presented nodules. I looked to the veins of the mesentery in addition, but did not see any enlargement or feel any hardness of them, and my conclusion was that the case was one of septic abscess of the liver, that the germs producing typhoid were conveyed directly through the venous circulation to the liver. I could not find any other explanation for it. There might have been originally multiple abscesses, their walls having broken down, and formed one large cavity.

With reference to amœbæ mentioned by Dr. Van Hook, seventeen or eighteen years ago that theory was advanced. Examinations were made in tropical climates where dysentery was common, and it was claimed that they were the cause of hepatic abscesses. We are apt to have these conditions more in tropical than in other climates.

I am positive that the symptoms of typhoid were as marked as in any case ever treated by me, with the exception of the absence of diarrhœa.

Dr. Graham: How early did you make the diagnosis?

Dr. Schaefer: I made it on the second day. The man came down gradually; he had a languid feeling a few days before, and when I came to examine his abdomen, it was extremely tender on pressure in the right iliac fossa, and as I pressed my ear down I heard the usual gurgling. There was *no hepatic tenderness* until the sixth or seventh week. The tongue was heavily coated from the onset, it was dry; there was constant thirst—in short, the patient had all of the general and most of the special symptoms of typhoid fever.

With reference to the question asked by Dr. Angear, I noticed that late in the course of the disease the stools were slate-colored at times. No diarrhœa. Earlier the stools contained mucus.

Dr. Angear: They were not white or clay-colored?

Dr. Schaefer: No. The abscess did not involve the entire structure of the liver while he was in my care. There was anasarca during the seventh or eighth week, and during the third month the patient was tapped by the physician in attendance for ascites, and a quart of ascitic fluid removed. The abscess was not opened by this procedure.

DOMESTIC CORRESPONDENCE.

To the Editor of the JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION:

Springfield, Mass., April 25, 1892.

I enclose copy of resolutions adopted at annual meeting of Hampden District Medical Society, held in this city Wednesday, April 20. The district society is one of the sub-divisions of the Massachusetts State Society. I also enclose comments of the lay press regarding same.

There is no question what the old and influential Hampden district medical society thinks of Dr. Keeley and his bichloride of gold cure. At the annual meeting, this week, resolutions were adopted by an almost unanimous vote, referring to the board of censors for action on the conduct of a member of the society, Dr. A. R. Rice, who has removed to Lexington to take charge of the new Keeley institute in that town. The utter contempt of the "regulars" for the Keeley "cure," which they declare is no secret, having been used for years, and the indignation provoked by the "degrading" act of a regular physician in associating himself with Keeley, are told as plainly in the following resolutions as the English language can make them. It will be seen that the society calls for a committee which shall recommend for expulsion any member whom it has found upon investigation to be using the bichloride "cure" or having anything to with it.

Being the first official notice of the Keeley method by a Massachusetts medical society, if not the first by any medical society in the Eastern States, this thundering denunciation comes like a thunderbolt from a clear sky, and will be an eye-opener to thousands in this city and vicinity, which has already furnished not a few patients to the Keeley institute at White Plains, N. Y. In case the condemnation is warranted, it is exceedingly timely, and the conservative old "Hampden district" society will have the gratitude of the community. It is quite possible that the occasion of this action was not alone Dr. Rice's move, but the effect which the bichloride treatment had on a well-known gentleman who died in this city a few weeks ago after a season at White Plains. The resolutions gain added significance when it is known that the meeting was the largest in attendance for several years, and the vote for adoption was almost unanimous. Following are the resolutions in full, which were referred to the board of censors for action:

WHEREAS, according to common and newspaper report and upon information and belief it is known that a member of this society and fellow of the Massachusetts medical society in regular standing, has, by associating himself with one of the most notorious impostors of this century, in the application and use of a remedy for the cure of inebriety, called "bichloride of gold," and whose exact composition it is pretended is known only by, and is the sole property of, a certain individual, and

WHEREAS, no such stable chemical combination is possible, and the substance actually used with so much secrecy and profit to the proprietor is and has been employed in suitable cases for years by regular physicians, who well know its limitations and dangers, and

WHEREAS, by associating himself with a regular physician this pretender hopes to gain prestige, and the quasi-indorsement of the regular profession, thus enabling him to delude the public, and

WHEREAS, the association of a regular physician in such a capacity is calculated to injure the public and is degrading to those who are in fellowship with such physician, and recognizing that naught but evil can finally result from trifling with moral or physical facts, and that it is better to cure rightly and really than wrongly and delusively," and that by the "humbuggery of secrecy, delusion and hypnotic suggestion," a far less number will, in the end, receive benefit, and

WHEREAS, it is the opinion of the members of this society that the use of the drugs, in the manner employed, for the cure of inebriety by the aforesaid impostor, produces a cerebral stimulation with intellectual disorders which are sometimes quite serious, together with other grave nervous troubles, themselves constituting a form of inebriety frequently leading to insanity and suicide, and a lowering of vitality rendering the patient less able to resist and recover from ordinary diseases, and

WHEREAS, in those cases of inebriety claimed to have been cured by means of this pretended secret method of treatment, it is our opinion that such cures resulted not *because* of said treatment, but *in spite* of it, and there seems little doubt that hypnotic suggestion played an important part in effecting said cures, and it is our opinion that in all of the so-called "cures" the result attained could have been better secured by improving the moral condition of the patient, by the use of tonics or hydrotherapeutics, regulating nervous action, and by attention to the digestive tract, without subjecting the patient to the dangers of another form of inebriety, and without the element of secrecy. It is, therefore,

Resolved, that this society hereby directs its president to refer this subject to a proper committee who shall, before the next regular meeting, ascertain if any member of this society has identified himself with the manufacture, sale, distribution or use of any secret remedy, contrary to the code of ethics under which this society is organized, and if so, that such member or members be recommended for expulsion from membership in this society at said next regular meeting.

A. O. SQUIER,
342 Main St. Censor Hampden Dist. Med. Soc.

BOOK REVIEWS.

THE HYDRIATIC TREATMENT OF TYPHOID FEVER, by CHR. SIHLER, M.D., Ph.D., Professor of Histology in the Medical Department of the Western Reserve University; formerly Fellow of the Johns Hopkins University and Assistant in the Biological Laboratory. Cleveland: Published by the Author.

Dr. Sihler has appeared before the profession before, as an advocate of the methods of Brand, in the treatment of typhoid fever. He certainly deserves great credit for his earnestness, but he is certainly misinformed as to the stand which the American profession has taken with regard to the treatment of typhoid fever by cold water. The method is well known to them and thoroughly understood by them, and if it has not obtained the extensive use which Dr. Sihler thinks that it ought to have, the reason is to be found in the peculiar practical nature of American physicians. The severely logical arguments of Dr. Sihler, which are too rigid to reach the whole truth in so flexible a subject as medicine, appeal more to the German than to the American mind. The exceedingly practical nature of American physicians, and their actual success as practitioners, ought to show Dr. Sihler that there is something more in medicine than the German philosophy yet dreams of. As a careful and faithful report of what has been done by Brand and his followers and of the possibilities of the hydriatic treatment of typhoid fever, the book is to be commended. Its perusal will repay any physician, and it is to be hoped that its circulation will be wide. The book is blemished by numerous typographical errors, and by some linguistic slips which betray the author's nationality, They do not, however, in any way diminish the actual value of the work.

GUY'S HOSPITAL REPORTS. Third series, Vol. xxxii. Edited by DRS. N. DAVIES-COLLEY and WM. HALE WHITE. 8 vo., pp. 369. London: J. & A. Churchill. 1891.

This presentation of the work done at Guy's is equal to the best of its series. It opens with a memorial note regarding the late Sir William Withey Gull, M.D., whose name appeared so frequently in the earlier volumes of these Reports. Gull's name was identified with Guy's Hospital from his earliest student days. Dr. Hale White's contribution on pernicious anæmia is based on the doctrine that this is a disease essentially distinct from and marked by the absence of the ordinary causes and concomitants of symptomatic anæmia. The proportion of recoveries among the cases treated at that hospital has been small. Not a few of those discharged cured have afterwards returned for further treatment, and some of these have died in hospital. Two cases, kept under observation a number of years, have not relapsed, and may be recorded as cured. Dr. White also writes on malignant diseases of the liver. The other editor, Dr. Davies-Colley, treats of malignant pustule, and his employment of ipecacuanha, internally and externally, with five successes in that number of patients; the neck was involved in three of these individuals. Dr. Golding Bird has a paper on congenital wry-neck, with remarks on facial hemiatrophy. Mr. A. H. Tubby writes on the pathology of acute infective periostitis and necrosis of growing bone. This is the report of investigations carried on in the workroom of the Royal College of Surgeons and the Leipsic Pathological Institute. Dr. James Goodhart gives one of a series of lectures on the pathology of cardiac bruits. Dr. J. W. Washbourn reports a case of glanders, with the results of experiments in inoculation and cultivation. The same author is joined in the contributions of the anatomists of the Hospital, and with Mr. Jacobson and Mr. Percy Evans in their antiseptic and antituberculous experiments. Mr. Clement Lucas has a case of uncomplicated fracture of the coracoid, and a study of that lesion. Dr. Goodall treats of chorea and compression paraplegia. The usual lists and routine information close the interesting volume.

WASHING THE HANDS.—For washing the hands previous to making vaginal examinations Prof. Parvin uses and very strongly recommends sapo viridis and sand. The sapo viridis gives a good lather, and the sand by mechanical action leaves the skin of the hands soft and in excellent condition for examination. This method is in general use in Germany, and since its introduction midwives have been required to wash their hands in this manner, and the mortality in obstetrical practice has been considerably reduced.—*Weekly Medical Review.*

MONOBROMIDE OF CAMPHOR FOR SPERMATORRHŒA.—The *Medical Summary* says: The monobromide of camphor has been successfully used in the treatment of spermatorrhœa, where a host of the usual remedies had been administered with no satisfactory results; finally the monobromide of camphor was given with prompt effect and perfect cures.

CAMPHORIC ACID IN ACUTE CORYZA.—A cotton-wool tampon charged with a 2 per cent. solution of camphoric acid, and introduced into the nostril, gives rapid and permanent relief in acute coryza.—*St. Louis Clinique.*

THE
Journal of the American Medical Association
PUBLISHED WEEKLY.

SUBSCRIPTION PRICE, INCLUDING POSTAGE:
PER ANNUM, IN ADVANCE $5.00
SINGLE COPIES . . . . . 10 CENTS.

Subscriptions may begin at any time and be sent to
THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION,
No. 68 WABASH AVE., CHICAGO, ILLINOIS.

All members of the Association should send their Annual Dues to the Treasurer, RICHARD J. DUNGLISON, M.D., Lock Box 1274, Philadelphia, Pa.

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SATURDAY, MAY 7, 1892.

FEES AND FEES.

Our attention is directed to a suit in a neighboring city brought by a couple of attorneys against the receiver of a defunct National Bank for professional services rendered in making certain collections. The professional labor was not claimed to be especially onorous, or as taking up a special length of time, but for the service a bill was rendered for $25,000. This the receiver declined to pay, hence the suit for that amount. The plaintiff summoned other barristers to testify as to the value of the service rendered. One testified that the bill was moderate, and should not have been less than $30,000. Another that the fee was low and should not have been less than $40,000. Some time ago the President of this same bank was tried for misappropriation of the funds of the bank, and to save himself a penitentiary sentence employed a couple of attorneys who required a $5,000 retainer, and $10,000 more before the case should be given to the jury. And we never heard that in either instance there was another of the legal profession ready to step on the witness stand and say the lawyers placed too high a value on their services.

Notwithstanding the adage that comparisons are odious, we will tell of a recent case in this city in which the value of medical services are called in question: The case was one of malignant diphtheria, the patient the only son of a wealthy man whose business is mainly conducted on the Board of Trade. The case was so severe as to indicate an almost certain fatal termination. To the father there was presented a case of loss or gain, the life of his only son was at stake. In his distress money was not thought of as a consideration, but of only one thing was he determined upon, and that the securing the very best skill to be found in the medical profession of Chicago. We do not know how he learned who one of the most experienced physicians was, but we will, without hesitation say he was properly directed to a gentleman whose skill is not surpassed in this or any other city, and whose observations and operations in this class of cases are so numerous and successful, as to place him in the very first rank among physicians who have made children's diseases a specialty. The man of means sent for the doctor of skill and repute, the case of desperation was placed in the doctor's hands; by continuous, watchful and most anxious service the life of the only boy was tided through the malady and restored in health and promise to the Board of Trade man.

In due time the physician sent in his bill for services rendered, not for $25,000 or $15,000, but for $2,000. The account was ignominously cast in the waste basket. Another bill was sent. The man with the large bank account characterized it as an attempt at robbery, and began sending around to different physicians to inquire the usual charge for performing the operation known as intubation, telling nothing of the particulars pertaining to this special case; fees were named. The miserly man of the large bank account felt that he had triumphed, here were the rates charged by other men for such operations, and he would pay no more. An arbitration was proposed, Moneybags would agree to this if he were allowed to name the arbitrator, but would not agree to allow the doctor to name one. Oh, my! it makes us weary to write of such a character. We will only say of him that he is now off in California traveling with that son whose life he said was not to be valued according to a money standard, but the doctor who saved him was to be rated at the salary of one of his clerks, and as an employe. In his opinion, only this and nothing more.

This is not a case for compromise on the doctor's part, but one for settlement either in court or by arbitrators. If by the latter, the men chosen should be of unquestioned standing in their own guild, and who are capable of appreciating the compensation which should follow the labor and continuous study of more than two decades, with its burning of barrel after barrel of midnight oil in persistent endeavor to acquire the skill, the necessary and coveted "know how," that is available on such an occasion as this, in order to save a hopeful boy from a narrow resting place in the city cemetery.

The bill was moderate and just, it should have been paid without a murmur, and accompanied by expressions of profound gratitude for a service that cannot be rated in value by a money standard. There is not a practicing physician in this city or in this land who is not personally interested in the payment of this and every other similar bill for professional service. It is by our service to others that we earn a livelihood, if such service is poorly compensated, it directly affects the current fees of every other man, and the lower the fees the lower the stan-

dard of the local profession in every other particular. Low fees mean a shoddy profession, low morals, with shallow attainments and the driving of bright minds into other callings. High fees mean culture, refinement, the highest attainable skill, self respect and laudable ambition.

Taking a retrospect of the past and a peep into the future: the man who is and would be known as reputable in his profession as a physician, should place a value on his services commensurate with his expenditure of a little fortune in money and the time and study of many years in attaining a skill and knowledge that will enable him to relieve painful suffering, cure diseases, prolong life, and direct such use of prophylactic measures as will prevent sickness.

SULPHURING FRUITS AND VEGETABLES.

Within the past few months, our attention has been directed to the process of sulphuring fruits and vegetables in order to beautify their appearance, by making them of a uniform light color, and removing indications of decay, all of which enabled the grower to place upon the market a commodity from which he could derive a much higher price.

From a commercial standpoint, the process transformed a nature's number two, three or four article into a more salable number one. The adulteration is comparable to the use of shoddy in the making of all wool and a yard wide cloth that won't shrink, and of cattle hair in the manufacture of warranted all long wool body brussels carpets.

There is, however, a very material difference. The deception in the sale of the shoddy all wool cloth that is a yard wide and won't shrink, only involves the purchaser in a financial loss in the item of wearing apparel, and so of the housekeeper in her parlor furnishing of warranted all long wool body brussels that is largely made up of cattle hair, her expectations of wear and retention of color are sadly disappointed, only this and nothing more, except a shaking of confidence in the word and integrity of the merchant.

In the case of the sulphured fruit there is, no doubt, in most instances, a deception practiced upon the innocent purchaser. This is quite bad enough, but there goes with the beautiful fruit a material amount of poison in the form of sulphate of zinc, sufficient in quantity to injure the health of those who partake of such fruits and vegetables.

None of these processes for preserving fruits are harmless, but of their dangerous nature the people are not informed, and for such reasons, the laws now pending in Congress, and known as the Pure Drug and Food Bill, and the one creating a Cabinet officer of Public Health, should be pressed to their passage. We have published copies of both these Bills, and hope the members of the American Medical Association and of all the State Societies are wide awake to their importance, and the necessity for active efforts in securing their enactment into laws.

THE DETROIT MEETING.

The local profession of Detroit are in harmony, and, as one man, united in their determination to make the ensuing meeting one grand success; in fact, that is already assured. The city itself is one of the most beautiful on the continent, and the first week in June is just about the best time in the whole year to see it to advantage. There will be neither sultry heat, nor blizzard cold, while the hospitality of the people is not second to those of any other city.

The accommodations are excellent, the hall and rooms for Sections are adjacent, while the hotels are all within convenient distance.

The Section officers report that the program cannot be improved, and that it will fairly reflect the progress of our art during the year that is past.

UNTOWARD EFFECTS FROM THE SALICYLATE OF SODIUM.

DR. F. W. MANN reports in the *Medical Record* some remarkable psychical symptoms following an overdose of this drug. The patient, a laborer of German birth, required treatment for a subacute rheumatic attack, for which was ordered a mixture containing two drachms of the salicylate in four ounces of mint water. This was to be taken in teaspoonful doses every second hour. Like many another German of his class, the patient thought that the more rapidly the remedy was ingested the more speedily the cure would be accomplished; he proceeded to swallow the whole four ounce mixture in the course of four hours. As the result of this overdose the patient began to have hallucinations, especially delusions of persecution. During the evening of that same day he became so troublesome to his friends that they felt constrained to call the aid of the police. The patient had to spend the night in a cell. The expectation was that the man would be consigned to a lunatic asylum on the following day. The history of the excessive dosing with the salicylic mixture having been taken into consideration, the man was sent to a hospital. During the next four days, the patient comported himself very much like a case of delirium tremens. Visual and auditory hallucinations possessed him. He refused all food, giving as his reason that he was soon to be hanged and that therefore it was useless to take nourishment. He gazed in a mirror and promptly struck it a blow with his fist, and broke it on account of the demoniacal visage which his own reflection brought to view. After this, physical restraint became expedient, and the man

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was kept in his bed. His attention was solely and constantly given to the thought of freeing himself from persecution and constraint. He was not coarse of speech nor rough in action. When spoken to he responded pleasantly. Respiration was not notably depressed, pulse 130. At the end of the fifth day the delusions gradually passed away and the man returned to his usual state of health. His rheumatism had disappeared also. The man will be more heedful hereafter to follow the instructions of his physician. Hallucinations are a not infrequent result of salicylism, but they have not been, as a rule, so long continued as in this instance with an overdose of the drug no greater than that here reported.

STATE MEDICAL SOCIETIES.

It is a matter of much gratification to note how generally the State societies are taking action on subjects brought before the American Medical Association at the last annual meeting. This is particularly the case with regard to petitions to Congress to enact the Bill now pending pertaining to the Cabinet Officer of Public Health.

The necessity for such an officer is so generally apparent that the proposition meets with universal commendation, where and whenever it has been broached in a medical society meeting.

The labors of DR. C. G. COMEGYS, Chairman of the Association Committee, have been untiring, and should be crowned with the success they so richly deserve.

The manner of taking up this subject by the State Societies and making the cause their own, aptly illustrates the possibilities of other important measures being furthered to a successful issue by a more compact and practical unification of the State Societies as actual branches of the American Medical Association. If the State Societies sustained such a relation, any measure pertaining to the weal of the medical profession, or to the betterment of the sanitary and hygienic conditions of the people, could emanate from any one of those organizations, or from the American Medical Association, and through the latter, secure the coöperation of all the branches, so that in this way the voices of more than fifty thousand physicians would be heard.

* * * * *

The trend of men's thoughts in our Republic is in the channel of unification in order to accomplish the greatest measure of success. This is exemplified in the persistent efforts of men and corporations engaged in similar undertakings, or industries; combinations, trusts, pools, systems, associations and unions are formed. This is for the purpose of lessening both competition and cost of production. It also enables the managers of such unions to obtain grants and franchises with the least possible friction.

A corresponding practical unification of the State medical societies as parts of one federation in the American Medical Association, would make possible many desires for advancement that now seem to be and are visionary. These are: a uniformity of many of the State laws, particularly those pertaining to sanitary conditions, immigration, medical education, registration of physicians, adulteration of foods and drugs, care of the defective and criminal classes, etc. These are subjects that should naturally come under the supervision of the medical profession.

Furthermore, in all these matters the people will become better informed, and will cheerfully seek the guidance and direction of those whose lives are given up to a study of these subjects, for they are a necessary segment in the Nation's wheel of political economy.

RHEUMATISM, HEART DISEASE, AND CHOREA.

The inter-relations of rheumatism, heart disease, and chorea, have been made the subject of a very interesting investigation by WALTON and VICKERY.[1] These gentlemen approached the subject with *a priori* feeling that the relations generally supposed to exist between these several conditions, were probably incorrect, but their results have confirmed, substantially, the current opinion.

Their study is based upon an analysis of 76 cases of chorea, which they saw together, and which have been studied with unusual care. They excluded chorea apparently due to hysteria, and habit chorea, as well as chorea due to gross organic disease of the brain. Careful distinction was made between functional and organic heart disease, and the diagnosis of cardiac lesion was not based upon a single sign, but upon the entire group of objective symptoms—"the size of the heart, the presence or absence of a thrill, the locations of the murmurs heard, and the direction in which they were transmitted, the accentuation or non-accentuation of the pulmonic second sound, and the evidences of venous stasis. As regards the question of rheumatism, vague pains were not included under rheumatism, even when situated in the joints, but the history of 'pain, heat, redness, and swelling,' was carefully sought, and only those cases included under rheumatism where the history was sufficiently accurate to render probable the existence of true rheumatic inflammation."

Their results may be classified in percentages as follows:

1. No possible rheumatism or cardiac disease either functional or organic	47.37
2. Rheumatism without heart disease	3.94
3. Rheumatism with irregular heart	6.58
4. Vague pains without rheumatism	17.10
5. Both heart disease and rheumatism	13.16
6. Organic heart disease without rheumatism	14.47
7. Functional heart disorder	14.47

[1] Amer. Jour. Med. Sciences, May, 1892.

The authors further state that their statistics show a total percentage of organic heart disease of 27.63, and a total percentage of rheumatism of 23.68, but this latter figure apparently omits item 2 of the above table, while including items 3 and 4.

Among their conclusions are the following:

1. Neither rheumatism nor heart disease is essential to chorea.

2. The preponderence of evidence points toward the conclusion not only that rheumatism and organic heart disease conjointly appear more frequently in the choreic subject than can be accounted for by coincidence, but that the same is true of each of these affections separately. It follows, therefore, that (*a*) rheumatism predisposes to chorea, and (*b*) organic heart disease has the same tendency.

4. There is a large class of functional cases (chorea), largely reflex, and fostered by circumstances, tending to produce functional symptoms in general.

5. The pathological connection between rheumatism and chorea, excepting in the cases where emboli are produced by accompanying endocarditis, is still obscure; probably no one theory is applicable to all cases.

6. The mechanism by which the peculiar phenomena of chorea are produced is unknown.

In defining accurately their lines of research, the authors have done well, as they give figures which can be satisfactorily compared with similarly made observations by others. But in their conclusions they use words without indication of the special limitation which they have previously put upon them, and which would be misleading if considered apart from these limitations. Chorea being essentially a disease of childhood, its relation to the rheumatism of that age is the problem to be considered. Pediatricians are quite well agreed that true rheumatism manifests itself in childhood in a very different shape from what it assumes in adult life. The disease is certainly milder in childhood, and the joint symptoms in particular are less marked. Therefore to develop the relations of chorea to rheumatism of the adult type in childhood, is to introduce a serious error.

We cannot admit, therefore, that the first conclusion of the authors is established. The authors have reviewed a great deal of literature in the preparation of their article, but we regret that they have apparently overlooked the highly interesting and valuable monograph of CHEADLE on "The Rheumatic State in Childhood."

In this little book, CHEADLE shows very conclusively that the three prominent symptoms of the rheumatic state, viz., the joint affection, cardiac disease, and chorea, may occur, and recur in any order, so that it is unjustifiable to call chorea a sequel of rheumatism or cardiac disease although that is the common order of manifestation. The latter part of the second conclusion of the authors, is therefore open to objection.

How large the class, to which they refer in their fourth conclusion may be, is open to considerable discussion.

Unfortunately the statistical method is not adapted to settle the question of the relation between the three phenomena under discussion. At best it can merely show that some relation between them does exist, but the extent, as well as the kind of the relation, must await the discovery of the common factor which binds them together.

VENEREAL STRICTURE OF THE RECTUM.

DR. C. B. KELSEY, in the *New York Medical Journal*, March 26, treats of some of the rarer diseases of the rectum that result in stricture or occlusion. He exhibited to his class a case of chancroidal ulceration around anus and within rectum. He regards this ulcerative condition as one of the occasional sources of so-called syphilitic stricture. It tends to confirm the classical argument of MASON, who, nearly twenty years ago, called attention to the frequency of rectal stricture due to chancroid, although generally known as syphilitic. Venereal stricture seems very often to have a doubtful causation, for the reason that the practitioner is unable to follow up the changes that occur. We sometimes see specific vaginitis assigned as a probable cause of the trouble in females. There are more cases of venereal stricture among females, and they are apt to be refractory against specific remedies. The exact mechanism of this form of stricture will be better understood after the consecutive histories of a considerable series of cases shall have been recorded and studied. This point is well to be borne in mind that a certain proportion of patients is strangely insensitive to rectal lesion. So that a case of alleged syphilitic stricture cannot fairly and definitely be set down to that etiology, if there has been an incomplete examination of rectal region at the time of the initial lesion. It is even asserted that patients, having rectal stricture, have not known it, but have merely surmised that they had some form of dysenteric trouble. While not denying the possibility of a genuine syphilitic causation for some of these strictures, there still remains the fact that neither mercury nor potassium iodide have had much if any restraining or remedial effect upon them, so that, in the present status of our knowledge, the term venereal stricture, rather than the more familiar term "syphilitic," should be the saving clause of our nomenclature; it is probably less misleading than the other and contains within it a suggestiveness that an early and full history of these cases is desirable.

QUESTIONS FOR CONSIDERATION.

The Committee appointed at the last meeting of the American Medical Association, to consider the best means for promoting the prosperity of the Sections of the Association, will hold an adjourned meeting in the Hotel Cadillac, Detroit, Mich., June 6, at 3 P.M.

The Committee would esteem it a favor if each member of the Association would communicate in writing his or her views concerning the best measures for promoting the development of the Sections. Such communications may be sent to the Chairman of the Committee, John S. Marshall, M.D., 9 Jackson St., Chicago.

1. Ought the Sections to occupy a more important place in the conduct of the affairs of the American Medical Association?

2. If they should, what is that place?

3. Do the Sections need an Executive Committee, like that possessed by the Section on Ophthalmology, to look after their individual interests and development?

4. Should the Section Executive Committee be formed of the ex-chairmen of the Sections, have a term of office of three years, the yearly vacancy being filled by the retiring chairmen?

5. Should each Section have yearly reprinted from THE JOURNAL its papers, discussions, list of membership, with the past and present officers, and its rules and regulations for the conduct of its affairs?

6. Should each Section have an annual dinner for the promotion of personal acquaintance among its membership?

7. Should the Sections have more time for the reading and discussion of their papers? If so, how much more time?

8. Should the Sections have any voice in the election of the officers of the general Association, in the selection of its place of meeting and in the general conduct of its affairs?

9. If they should have such a voice, in what manner could they use it to the best interest of all concerned?

10. Would it be wise to organize the Executive Committee of the Sections — if so formed — into a general Business Committee of the Association, to whom should be referred all business matters needing consideration by other than existing committees, and to whom should be delegated the nomination of the general officers of the Association, the selection of the place of meeting, etc.?

11. Would it advance the interests of the Sections to so arrange matters that, with the exception of one hour, the entire working day of the Association meetings be given to Section work? the excepted hour to be devoted each day to the general Association meetings, and the evenings to social relaxation.

12. Would it advance the value and interest of the Sections if it was understood that the Section meetings formed the leading feature of the meetings of the American Medical Association?

13. Is it wise to endeavor to have each Section choose an Executive Committee after the model suggested above, with specific instructions to look after the development of each Section by the most practicable methods?

14. Is it wise for the several Executive Committees to meet together as soon as convenient after being formed, and discuss measures by which the common interests of all the Sections shall be advanced? Would it be wise for these to form a permanent organization to secure such changes in the conduct of the affairs of the general Association as will promote their best individual and collective development?

EDITORIAL NOTES.

"TYPHOID FEVER" AT WINTERING RESORTS.—Fever is said to have declared itself at St. Augustine, and Tampa Bay, Florida, but whether it is genuine typhoid fever or a typho-malarial manifestation is not certain. There have been some hasty departures from the tourist's wintering places and a circumscribed scare has been created, affecting the good name of some of the hotels in regard to which the utmost sanitary vigilance and diligence are supposed to be exercised. No deaths have been reported to have followed these attacks of so-called typhoid fever. The late staying northerners, or the most susceptible among them, should not expect exemption from malarial or typho-malarial influences, in a country concerning which it has been satirically said that, in some districts, "building sites are sold at so much a gallon."

DEATH OF A "GLASS-EATER."—The *Medical Fortnightly*, April, refers to the fatal result of an operation on a patient at the St. Louis City Hospital, who for eight years had been a glass and nail swallower at the dime museum exhibitions of that city. On March 24, a gastrotomy was done for the relief of extreme pain and emaciation. A considerable mass, weighing over a pound, was taken from the stomach. It contained pieces of glass, shingle nails, sharp pointed tacks, and screws of various sizes; and this after an amount, not weighed, of like material had been gotten rid of by emesis. The stomach gave evidence of having been a normally developed organ before the patient entered upon the career of a "freak," and began to "eat glass" for a livelihood. He is said to have been able to follow his calling for nearly eight years, which is an exceptionally long lease of life in that hazardous business. The patient succumbed within twelve hours after the operation.

TUBERCULOUS DISEASE OF THE UVULA.—The *American Journal of the Medical Sciences* has a note on the above condition, subsequent to a "cure" of tuberculous larynx, which latter had occurred in the fourth year of a pulmonary tuberculosis. There was bilobar hypertrophy of the uvula with tubercles, at the center of each lobe. Dysphagia was extreme.

ACETANILID.—Dr. F. W. Lester, of David City, Nebraska, in *Times and Register*, calls attention to a peculiarity not formerly noted as to the above drug. He has taken it frequently as a remedy for the headache, and in ten to thirty minutes after taking it, and then going out into the open air, he observes a delightful odor not unlike that of the tuberose. Antikamnia produces the same effect, but neither antipyrin nor phenacetin has the like property. It is undoubtedly an idiosyncrasy or phenomenon having a limited range. We know of several frequent takers of the drugs acetanilid and its congener antifebrin, who have not experienced the subjective tuberose symptom.

AN M.D. SELF-QUALIFIED.—There is a sharp-witted dentist in New York City who is liable to get into legal trouble through his smartness. His name is reported as Alexander Walter, he claims to have been in dental practice twenty years, and he writes "M.D." after his name. He was arrested on four charges that he was engaged in the practice of medicine without license or diploma. In his statement he entered the plea that the letters M.D. were used to show that he was a "Mechanical Dentist," and that he had never represented himself as a doctor of medicine. His trial is still on.

LACERATION OF VAGINA DURING LABOR.—Piering has reported a case of the above named complication in a primipara, in *Centralblatt für Gynækologie*, November, 1891. The patient, aged 41, in labor at full term, had the head of child delayed at perineum on account of great rigidity of the soft parts. Instrumental delivery appeared not to be necessary, so that it was found expedient to make an incision one inch in length in the tough and inelastic perineal barrier, on right side. During the next pain a gush of blood came from the rectum. The right hand of the fœtus, and part of the forearm, came into view through the anus. The arm was replaced at once, and the child was born without additional laceration. The tissues nearest to the tear in the vagina were so badly contused that repair by first intention was out of the question. The incision in the perineum was not the seat of laceration. The uterine cervix was not torn. A thorough disinfection of the vagina was attempted, and iodoform gauze was applied in the form of a plug. On the third day the bowels were moved; no feces came out by way of the vagina. Two hyperpyrexiæ periods were all that marked the patient's convalescence. These disappeared when the torn surfaces were painted with tincture of iodine. The wound healed with a strong and firm cicatrix, which could not be discerned when examining from the rectal aspect.

THE late Ohio Legislature, with considerable merriment, rejected a bill to regulate the practice of medicine in the State, principally because it would interfere with itinerant doctors, and diminish the advertising receipts of some papers. This is a habit of protecting a class at the expense of the community, which some legislatures have. It did not adjourn however, without effecting any medical legislation. It appropriated $5,000 to test the efficacy of the Keeley cure, and made itself a sort of committee of the whole to investigate. Each member is to have the privilege of sending one patient. How many will take a personal advantage of this chance?

THE MONUMENT TO BENJAMIN RUSH.—The attention of the members of the medical profession is again called to the monument to Dr. Benjamin Rush, the great patriot physician of the Revolution, which it is proposed to erect in the city of Washington among those of the other illustrious founders of the Republic. In the person of this eminent medical man—"the greatest physician this country has ever produced," as he has been fitly styled by a very eminent physician, happily still living—*Medicine* will be worthily represented at the National Capital as a profession, which can boast of having public spirited citizens, fearless patriots, brave warriors, and wise legislators among its ranks. Benjamin Rush was, himself, all these. The members of the profession, who have not already done so, are urgently solicited to come to Detroit to attend the meeting of the American Medical Association, prepared to contribute towards the erection of this monument, or to send their subscriptions *at once* to Dewit C. Patterson, M.D., Treasurer, 919 I Street, N. W., Washington, D. C. ALBERT L. GIHON, M.D., Chairman.
GEORGE H. ROHE, M.D., Secretary.

NOTICE.—The third annual session of the Association of American Medical Colleges will convene at the Building of the Detroit College of Medicine, at 3 o'clock P.M., Wednesday, June 8, 1892. Dr. N. S. Davis, President of the Association, will read a paper upon the following named subject, to-wit: To what extent should clinical instruction be afforded the student of Medicine in regular course? Another paper will be read by Prof. V. C. Vaughan, of the University of Michigan, as follows: To what extent should laboratory instruction be afforded the student of Medicine in regular course? The discussion upon the papers will be opened by parties selected by the authors, and continued by the delegates in extenso. The indications point to a large attendance and a

most interesting session, with the probability of representatives from a large majority of the colleges of the United States.

THE BACILLUS OF MEASLES.

DR. P. CANON AND DR. W. PIELICKE.

In the well-known Berliner hospital—The Moabit Barracks—of which Dr. P. Guttmann is the head, experiments have been made by Dr. P. Canon (the discoverer of the influenza bacillus) and Dr. W. Pielicke culminating in the discovery of a bacillus in the blood of measles. I herewith send a translation of their article as it will appear in the current number of the *Berliner Kleinische Wochenschrift:*

Up to the present time but few bacteriological examinations concerning measles have been made public.

In most cases the pneumonic affected lung of measles was examined after death and various authors found cocci, arranged partly in masses, partly in chains. Thus Thaon in a pneumonic afflicted lung of measles found multitudes of diplococci and bacilli. Thus also Babes (Cornil et Babes, Les Bacteries, Paris, 1886, pages 621, 632) found in the infected lungs, lymphatic glands, nasal mucus, secretion of conjunctiva, and in the exudate of the papule, cocci, singly or in pairs forming the figure of 8, and arranged also in chains. Moreover, he examined blood obtained from the papule of measles and found, likewise, the above mentioned cocci; in one case he observed very short bacilli. In cultures he obtained streptococci which bore resemblance to the streptococcus pyogenes.

At the suggestion of Dr. P. Guttmann, the blood of fourteen patients ill with measles, was examined in colored preparations and in *all cases we found the one and same kind of bacillus.*

The cultures were prepared in the same manner as the ones used for influenza bacillus (Deutsche Medicine Wochenschrift, 1892, No. 11), and colored with a solution of eosin-methylene blue. We used mostly a solution which contained less eosin in order that a weaker supplemental color might be obtained.

Concentrated aqueous solution of methylene blue, 40.0; ¼ per cent. solution eosin (in 70 per cent. alcohol) 20.0; distilled water, 40.0. The preparations were laid in absolute alcohol from 5 to 10 minutes, then colored in the incubator at 37° C. from 6 to 20 hours; the examination was made with the aid of the movable objective of Zeiss.

The following solution proved also of benefit: Concentrated aqueous solution of methylene blue 80.0; ¼ per cent. eosin solution (in 70 per cent. alcohol), 20.0. With this solution two or three hours were sufficient to color in the incubator and all *bacilli were colored blue.* Sometimes they were colored throughout, then again the ends took on a stronger color while the middle part was paler, while at other times the outlines of the middle portions alone took the color.

The size of the bacillus varies. Often they attain the radius of a red corpuscle, others again are very small and then appear as double cocci; between these two forms there are multifarious gradations in size. Frequently, single ones are met with that are distinctly longer than broad so that one has the impression of a double bacillus or a bacillus divided in half. This form has also various sizes.

Sometimes the bacillus has an unusual size, being longer than the radius of a red corpuscle and often attaining the length of the diameter of a red corpuscle. Furthermore they do not show a like coloring throughout but have three or four spots not colored, these alternating with colored portions. In general the coloring obtained by these forms is not a strong one, the ends often showing a very weak blue. Finally, they are frequently easily bent, and appear only in cultures which were taken toward the end of the fever (on the sixth day).

We hold that the *bacillus found by us in the living blood of the fourteen cases of measles are alike, and we see in them the exciting cause of the disease.*

These bacilli are found in the blood of measles in various quantities. We found on several occasions in two or three preparations taken from the blood but few bacilli, while at other times the very first field was crowded.

There were plenty single ones, and in most of the cases (12) masses of from 8 to 20 single ones could be traced.

The position of the bacilli to one another was not characteristic; though they showed an inclination to arrange themselves parallel to one another, still others lay close behind each other forming obtuse angles.

As regards the time; the bacilli were found during the whole course of the measles, in one case three days after the fever had disappeared. In later cases at the period of the crisis, they were present in particularly large quantities. While at the time of the disappearance of fever the most were then discovered.

Aside from these fourteen cases we examined the blood of seven children who were just convalescing from the measles, and where the exanthema were beginning to pale. In these seven cases the examination resulted negatively. We also examined the blood of a child dead from measles, ten hours after. This death was directly due to measles (without any complication of lung); we could not determine any bacilli with certainty, nor was the child's blood examined while yet alive.

Several cultures were colored according to Gram—the bacilli remained uncolored and appeared just as pale as the red blood corpuscles.

The bacilli as found in the blood were also seen in the *excretions and secretions of the nose and conjunctiva.* In all cases we attempted to make the culture by taking the blood from a punctured finger and inoculating glycerine agar, blood- serum, or milk (we used mother's milk). We did not succeed in our efforts to breed the bacilli of the blood in these soils. Later on we applied ourselves chiefly with bouillon, inoculating from one to three drops of blood; generally from six to ten bouillon-glasses were used.

In three cases bacilli were found in the blood-impregnated bouillon whose form agreed with the bacilli found in the blood cultures and from which no further cultures could be made in glycerine-agar, blood serum or bouillon. At first the bouillon remained clear; below a sediment was found which was partly due to the inoculated blood; then after several days there was noticed a faint cloudiness and small flakes formed, which, by agitating the glass, rose to the top. In these bouillon cultures were also found bacilli in various forms, at times symmetrically colored resembling, sometimes, diplöcocci, and at other times double-bacilli. Some single ones were found, which in length surpassed those which we had seen in the blood cultures; most of them were easy to recognize as they were made up of several frames which lay behind each other, at times they changed in position from each other and formed obtuse angles. The bacilli in these bouillon cultures did not color themselves according to Gram and appeared to have a slight movement of their own.

In one of the three cases mentioned (there were four), in all the bouillon-glasses which were inoculated with the blood these bacilli were found in large quantities; inoculations in glycerine-agar and blood-serum, done at the same time, remained sterile. The blood inoculations in these cases were done near the end of the fever process, at the beginning of the crisis; in the blood cultures prepared at the same time

the bacilli were also found in large quantities. Moreover, it was discovered that three days after the fever process had ended, the bacilli still existed in the blood.

A few days prior we obtained blood from the same children; this was placed in sterile tubes which, after two days in the incubator, were made into cultures with bouillon, and also with agar. In one of the bouillon glasses we found bacilli, while the agar remained sterile. Here also attempts at further culture proved futile. Following this method, Bruschettini cultivated the influenza bacillus out of the living blood (*Riforma Medica*, n. 23, Gennaio, 1892). Often, in the course of our examination, we utilized this method, but aside from this one case, without result.

In the other two cases where the bacilli from the living blood could be cultivated in bouillon, the inoculation was performed during the fever process, and in only one or two glasses could bacilli be found; all the other glasses remained sterile.

On the other hand, in two other cases where the blood was inoculated during the fever process, no bacilli were found in the bouillon glasses.

The same negative result ensued in a number of blood inoculations which were taken one or two days after the fever process had ceased. In one cultivation at any rate, taken from a blood-inoculated bouillon-glass, after the same had been sufficiently shaken, and by the aid of a movable objective, we found, after diligent search, from two to five bacilli; though it is questionable to conclude from this that there is an increase of bacilli in the bouillon culture.

In one case, ten hours after a death due directly to measles, plentiful inoculations were made in different cultures, as also in bouillon. All remained sterile.

Finally, the experiment was tried to cultivate the bacilli in blood-serum glycerine-agar, after the method of Wertheim, who advised the same in gonococci cultures (*Deutsch. Medic. Wochenschr.*, 1891, No. 50). These attempts, up to the present, proved futile. In this case the blood-serum was obtained from a person who was said to have had severe measles seven years prior. It might have been more advisable to use as a breeding soil the blood of a person who never had measles, and therefore who has had no certain immunity from the disease.

These bacilli found by us in measles, *discriminate themselves substantially from those microörganisms which have ever been described up to this time.* It is possible that the one case of Babes of the bacillus seen in the blood, "*bacilles très courts*," of which he furnished no description, may be identical with our bacillus, but as he only found cocci (microbes ronds), he does not mention in how many cases—that lay singly, mostly also in pairs, and often formed chains—therefore no further consideration need be taken of this discovery, on which he himself appears to lay no stress.

Translated by DR. JACOB ROSENTHAL.

Berlin, April 16, 1892.

MISCELLANY.

THE ILLINOIS ARMY AND NAVY MEDICAL ASSOCIATION.—Annual meeting will be held at Vandalia, May 19–20, 1892.

Officers—President, John H. Rauch, Chicago; First Vice-President, Henry W. Kendall, Quincy; Second Vice-President, Robert Roskoten, Peoria; Third Vice-President, Archibald Agnew, Samoth; Fourth Vice-President, Edmund Andrews, Chicago; Fifth Vice-President, A. C. Pickett, Mattoon; Secretary, Edward P. Bartlett, Springfield; Treasurer, W. J. Chenowith, Decatur.

Historical Committee—John H. Hollister, Chicago; John D. Skeer, Chicago; Z. P. Hanson, Chicago; R. M. Lackey, Oak Park; J. W. Green, Marengo; H. T. Godfrey, Galena; Edward Gaylord, Magnolia; J. C. Corbus, Mendota; H. A. Kelso, Paxton; E. L. Phillips, Galesburg; S. C. Plummer, Rock Island; W. C. Day, Winchester; P. L. Dieffenbacher, Havana; I. N. Barnes, Decatur; Lyman Hall, Champaign; Z. D. French, Sumner; J. P. Matthews, Carlinville; E. Gulich, Alton; H. C. Plummer, Mt. Vernon; Ford S. Dodds, Anna.

Committee on By-Laws—W. O. Ensign, Rutland; D. H. Law, Dixon; P. L. Dieffenbacher, Havana.

The Illinois State Medical Society will meet at Vandalia, May 17–18–19, 1892, and the usual reduction in railroad fare is promised, on the certificate plan. The certificate should be secured when the ticket is bought at the local railroad office.

The Association will be called to order on Thursday forenoon, and remain in session during Friday, May 20, thus enabling members to reach their homes before the Sabbath.

EDWARD P. BARTLETT, Secretary.

ASSOCIATION OF AMERICAN PHYSICIANS.—The following is the programme of the Seventh Annual Meeting of the Association of American Physicians, to be held in the Army Medical Museum and Library Building, corner Seventh and B streets, Washington, D. C., May 24, 25 and 26, 1892:

Tuesday, May 24. Morning session, 10 A.M.—1. President's Address, by Henry M. Lyman, Chicago, Ill.; 2. General Business; 3. The Cold Water Treatment of Typhoid Fever, by G. Wilkins, Montreal; 4. The Treatment of Follicular Tonsillitis, by G. M. Garland, Boston; 5. A Collective Investigation in Regard to the Value of Quinine in Malarial Hæmaturia or Malarial Hæmoglobinuria, by H. A. Hare, Philadelphia; 6. Alcoholism, by T. S. Latimer, Baltimore.

Afternoon session, 2.30 P.M.—7. Practical Results of Bacteriological Researches, by G. M. Sternberg, U. S. N.; 8. The Treatment of Experimental Tuberculosis by Koch's Tuberculin, Hunter's Modifications, and other products of the Tubercle Bacilli, by E. L. Trudeau, Saranac Lake; 9. Report of a Case of Glanders, with Results of Bacteriological Study, by William Pepper, Philadelphia; 10. The Bacteriological Study of Drinking Water, by V. C. Vaughan, Ann Arbor; 11. The Morbid Anatomy of Leprosy, by Heneage Gibbes, Ann Arbor, Mich.

Wednesday, May 25. Morning session, 10 A.M.—12. Discussion on Dysentery—Etiology and Pathology; Referee, W. T. Councilman, Baltimore; Symptomatology, Complications and Treatment; Co-Referee, A. B. Ball, New York; 13. Treatment of Acute Dysentery by Antiseptic Colon and Rectal Irrigation, by W. W. Johnston, Washington; 14. A Contribution to the Study of Hepatic Abscess, by W. C. Dabney, Virginia; 15. Pulsating Pleural Effusions, by James C. Wilson, Philadelphia.

Afternoon session, 2.30 P.M.—16. A Case Presenting the Symptoms of Landry's Paralysis, with Recovery, by F. T. Miles, Baltimore; 17. A Case Showing Symptoms of Landry's Paralysis—Recovery, by A. McPhedran, Toronto, Canada; 18. The Areas of Anæsthesia in Spinal Cord Lesions as a Guide to Localization, by M. A. Starr, New York; 19. A Study of the Seasonal Relations of Chorea and Rheumatism for a Period of Fifteen Years, by Morris J. Lewis, Philadelphia; 20. The Significance of Intermission in Functional Nervous Diseases, by W. H. Thomson, New York.

Thursday, May 26. Morning session, 10 A.M.—21. Misconceptions and Misnomers Revealed by Modern Gastric Research, by Charles G. Stockton, Buffalo; 22. The Production of Tubular Breathing in Consolidation and other Conditions of the Lungs, by Charles Cary, Buffalo; 23. The Different Forms of Cardiac Pain, by Samuel G. Chew, Baltimore; 24. The Late Systolic Murmur, by J. P. Crozer Griffith, Philadelphia; 25. Tube Casts and Their Diagnostic Value, by I. N. Danforth, Chicago; 26. Studies in Hypnotism, by B. F. Westbrook, Brooklyn; 27. Influenza, and Some of its Present Aspects, by Morris Longstreth, Philadelphia; 28. Concluding Business.

The annual dinner of the Association will be held on the evening of Wednesday, May 25, at 8 o'clock, in the Arlington Hotel.

OFFICIAL LIST OF CHANGES in the Stations and Duties of Officers Serving in the Medical Department, U. S. Army, from April 23, 1892, to April 29, 1892.

Capt. John L. Phillips, Asst. Surgeon U. S. A., is granted leave of absence for one month, to take effect on the final adjournment of the board of officers convened by Par. 1, S. O. 32, c. s., Hdqrs. Dept. of Missouri.

Capt. Benjamin Munday, Asst. Surgeon U. S. A., is granted an extension of one month to leave of absence granted in S. O. 40, Dept. Dak., March 19, 1892. S. O. 98, A. G. O., April 26, 1892.